

OBSERVATIONS ON  
DIFFERENT KINDS OF  
AIR. ... BY JOSEPH  
PRIESTLEY, LL.D.  
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Joseph Priestley, James Basire





*Observations on different Kinds of Air.* By  
Joseph Priestley, *L.L. D.F. R. S.*

Read March 5, 1772. THE following observations on the properties of several different kinds of air, I am sensible, are very imperfect, and some of the courses of experiments are incomplete; but a considerable number of facts, which appear to me to be new and important, are sufficiently ascertained; and I am willing to hope, that when philosophers in general are apprized of them, some persons may be able to pursue them to more advantage than myself. I therefore think it my duty to give this Society an account of the progress I have been able to make; and I shall not fail to communicate any further lights that may occur to me, whenever I resume these inquiries.

In writing upon this subject, I find myself at a loss for proper terms, by which to distinguish the different kinds of air. Those which have hitherto obtained are by no means sufficiently characteristic, or distinct. The terms in common use are, fixed air, mephitic, and inflammable. The last, indeed, sufficiently characterizes and distinguishes that kind of air which takes fire, and explodes on the approach of flame; but it might have been termed fixed with

as much propriety as that to which Dr. Black and others have given that denomination, since it is originally part of some solid substance, and exists in an unelastic state, and therefore may be also called factitious. The term mephitic is equally applicable to what is called fixed air, to that which is inflammable, and to many other kinds; since they are equally noxious, when breathed by animals. Rather, however, than to introduce new terms, or change the signification of old ones, I shall use the term fixed air, in the sense in which it is now commonly used; and distinguish the other kinds by their properties, or some other periphrasis. I shall be under a necessity, however, of giving a name to one species of air, to which no name was given before.

#### Of FIXED AIR.

Fixed air is that which is expelled by heat from lime, and other calcareous substances, and, when deprived of which, they become quick-lime. It is also contained in alkaline salts, and is generated in great quantities from fermenting vegetables; and being united with water, gives it the principal properties of Pyrmont-water. This kind of air is also well known to be fatal to animals; and Dr. Macbride has demonstrated, that it checks or prevents putrefaction.

Living for some time in the neighbourhood of a public brewery, I was induced to make a few experiments on this kind of air, there being always a large body of it, ready formed, upon the surface of the fermenting liquor, generally about nine inches or

or a foot in depth, within which any kind of substance may be very conveniently placed ; and though it must be continually mixing with the common air, and is far from being perfectly pure, yet there is a constant supply from the fermenting liquor, and it is pure enough for many purposes.

A person, who is quite a stranger to the properties of this kind of air, would be agreeably amused with extinguishing lighted candles, or chips of wood in it, as it lies upon the surface of the fermenting liquor ; for the smoke readily unites with this kind of air, probably by means of the water which it contains ; so that very little or none of the smoke will escape into the open air, which is incumbent upon it. It is remarkable, that the upper surface of this smoke, floating in the fixed air, is smooth, and well defined ; whereas the lower surface is exceedingly ragged, several parts hanging down to a considerable distance within the body of the fixed air, and sometimes in the form of balls, connected to the upper stratum by slender threads, as if they were suspended. The smoke is also apt to form itself into broad flakes, parallel to the surface of the liquor, and at different distances from it, exactly like clouds. These appearances will sometimes continue above an hour, with very little variation. When this fixed air is very strong, the smoke of a small quantity of gunpowder fired in it will be wholly retained by it, no part escaping into the common air.

Making an agitation in this air, the surface of it, which still continues to be exactly defined, is thrown into the form of waves, which it is very amusing to look upon ; and if, by this agitation, any of the fixed air

air be thrown over the side of the vessel, the smoke, which is mixed with it, will fall to the ground, as if it was so much water, the fixed air being heavier than common air.

The red part of burning wood was extinguished in this air, but I could not perceive that a red-hot poker was sooner cooled in it.

Fixed air does not instantly mix with common air. Indeed, if it did, it could not be caught upon the fermenting liquor; for a candle put under a large receiver, and immediately plunged very deep below the surface of the fixed air, will burn some time. But vessels with the smallest orifices, hanging with their mouths downwards in the fixed air, will in time have the common air, which they contain, perfectly mixed with it. When the fermenting liquor is contained in vessels close covered up, the fixed air is rendered much stronger, and then it readily affects the common air which is contiguous to it; so that, upon removing the cover, candles held at a considerable distance above the surface will instantly go out. I have been told by the workmen, that this will sometimes be the case, when the candles are held more than half a yard above the mouth of the vessel.

Fixed air unites with the smoke of resin, sulphur, and other electrical substances, as well as with the vapour of water; and yet, by holding the wire of a charged phial among these fumes, I could not make any electrical atmosphere, which surprized me a good deal, as there was a large body of this smoke, and it was so confined, that it could not escape me. I also held some oil of vitriol in a glass vessel within the

the fixed air, and by plunging a piece of red hot glass into it, raised a copious and thick fume. This floated upon the surface of the fixed air like other fumes, and continued as long.

Considering the near affinity between water and fixed air, I concluded that if a quantity of water was placed near the yeast of the fermenting liquor, it could not fail to imbibe that air, and thereby acquire the principal properties of Pyrmont, and other medicinal mineral waters. Accordingly, I found, that when the surface of the water was considerable, it always acquired the pleasant acidulous taste that Pyrmont water has. The readiest way of impregnating water with this virtue, in these circumstances, is to take two vessels, and to keep pouring the water from one into the other, when they are both of them held as near the yeast as possible ; for by this means a great quantity of surface is exposed to the air, and the surface is also continually changing. In this manner, I have sometimes, in the space of two or three minutes, made a glass of exceedingly pleasant sparkling water, which could hardly be distinguished from very good Pyrmont.

But the most effectual way of impregnating water with fixed air is to put the vessels which contain the water into glass jars, filled with the purest fixed air, made by the solution of chalk in diluted oil of vitriol, standing in quicksilver. In this manner I have, in about two days, made a quantity of water to imbibe more than an equal bulk of fixed air, so that, according to Dr. Brownrigg's experiments, it must have been much stronger than the best imported Pyrmont ; for though he made his experiments at the spring head,

head, he never found that it contained quite so much as half its bulk of this air. If a sufficient quantity of quicksilver cannot be procured, oil may be used with sufficient advantage, for this purpose, as it imbibes the fixed air very slowly. Fixed air may be kept in vessels standing in water for a long time, if they be separated by a partition of oil, about half an inch thick. Pyrmont water made in these circumstances, is little or nothing inferior to that which has stood in quicksilver.

The *readiest* method of preparing this water for use is to agitate it strongly with its whole surface exposed to the fixed air. By this means also, more than an equal bulk of air may be communicated to a large quantity of water in the space of a few minutes. Easy directions for doing this I have published in a small pamphlet, designed originally for the use of seamen in long voyages, on the presumption that it might be of use for preventing or curing the sea scurvy, equally with wort, which was recommended by Dr. Macbride for this purpose, on no other account than its property of generating fixed air, by its fermentation in the stomach.

Water thus impregnated with fixed air readily dissolves iron, as Mr. Lane has discovered; so that if a quantity of iron filings be put to it, it presently becomes a strong chalybeate, and of the mildest and most agreeable kind.

I have recommended the use of chalk and oil of vitriol as the cheapest, and, upon the whole, the best materials for this purpose; and whereas some persons had suspected that a quantity of the oil of vitriol was rendered volatile by this process, I examined it

by all the chemical methods that are in use; but could not find that water thus impregnated contained the least perceptible quantity of the acid.

Mr. Hey, indeed, who assisted me in this examination, found that distilled water, impregnated with fixed air, did not mix so readily with soap as the distilled water itself; but this was also the case when the fixed air had passed through a long glass tube filled with alkaline salts, which, it may be supposed, would have imbibed any of the oil of vitriol that might have been contained in that air\*.

It is not improbable but that fixed air itself may be of the nature of an acid, though of a weak and peculiar sort. Mr. Bergman of Upsal, who honoured me with a letter upon the subject, calls it the aerial acid, and, among other experiments to prove it to be an acid, he says that it changes the blue juice of tournesole into red.

The heat of boiling water will expell all the fixed air, if a phial containing the impregnated water be held in it; but it will often require above half an hour to do it completely.

Dr. Percival, who is particularly attentive to every improvement in the medical art, and who has thought so well of this impregnation as to prescribe it in several cases, informs me that it seems to be much stronger, and sparkles more, like the true Pyrmont water, after it has been kept some time. This circumstance, however, shews that, in time, the fixed air is more easily disengaged from the water, and

\* An account of Mr. Hey's experiments will be found in the Appendix to these papers.

though, in this state, it may affect the taste more sensibly, it cannot be of so much use in the stomach and bowels, as when the air is more firmly retained by the water, though, in consequence of it, it be less sensible to the taste.

By the process described in my pamphlet, fixed air may be readily incorporated with wine, beer, and almost any other liquor whatever; and when beer, wine, or cyder, is become flat or dead (which is the consequence of the escape of the fixed air they contained) they may be revived by this means; but the delicate and agreeable flavour, or acidulous taste, communicated by fixed air, and which is very manifest in water, can hardly be perceived in wine, or any liquors which have much taste of their own.

I should think that there can be no doubt, but that water thus impregnated with fixed air must have all the medicinal virtues of genuine Pyrmont water; since these depend upon the fixed air it contains. If the genuine Pyrmont water derives any advantage from its being a natural chalybeate, this may also be obtained by providing a common chalybeate water, and using it in these processes, instead of common water.

Having succeeded so well with this artificial Pyrmont water, I imagined that it might be possible to give ice the same virtue, especially as cold is known to promote the absorption of fixed air by water; but in this I found myself quite mistaken. I put several pieces of ice into a quantity of fixed air, confined by quicksilver, but no part of the air was absorbed in two days and two nights; but upon bringing it into a place where the ice melted, the air

was absorbed as usual. I then took a quantity of strong artificial Pyrmont water, and, putting it into a thin glass phial, I set it in a pot that was filled with snow and salt. This mixture instantly freezing the water that was contiguous to the sides of the glass, the air was discharged plentifully, so that I catched a considerable quantity, in a bladder tied to the mouth of the phial. I also took two quantities of the same Pyrmont water, and placed one of them where it might freeze, keeping the other in a cold place, but where it would not freeze. This retained its acidulous taste, though the phial which contained it was not corked; whereas the other, being brought into the same place, where the ice melted very slowly, had at the same time the taste of common water only. That quantity of water which had been frozen by the mixture of snow and salt, was almost as much like snow as ice, such a quantity of air bubbles were contained in it, by which it was prodigiously increased in bulk.

The pressure of the atmosphere assists very considerably in keeping fixed air confined in water; for in an exhausted receiver, Pyrmont water will absolutely boil, by the copious discharge of its air. This is also the reason why beer and ale froth so much *in vacuo*. I do not doubt, therefore, but that, by the help of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring, and it would not be difficult to contrive a method of doing it.

The manner in which I made several experiments to ascertain the absorption of fixed air by different fluid substances was to put the liquid into a dish,

and holding it within the body of the fixed air at the brewery, to set a glass vessel into it, with its mouth inverted. This glass being necessarily filled with the fixed air, the liquor would rise into it when they were both taken into the common air, if the fixed air was absorbed at all.

Making use of ether in this manner, there was a constant bubbling from under the glass, occasioned by this fluid easily rising in vapour, so that I could not, in this method, determine whether it imbibed the air or not. I concluded, however, that they did incorporate, from a very disagreeable circumstance, which made me desist from making any more experiments of the kind. For all the beer, over which this experiment was made, contracted a peculiar taste, the fixed air impregnated with the ether being, I suppose, again absorbed by the beer. I have also observed, that water which remained a long time within this air has sometimes acquired a very disagreeable taste. At one time it was like tar-water. How this was acquired, I was very desirous of making some experiments to ascertain, but I was discouraged by the fear of injuring the fermenting liquor. It could not come from the fixed air only.

Having imagined that fixed air coagulated the blood in the lungs of animals, and thereby caused instant death; I suffocated a cat in this kind of air, and examining the lungs presently after, found them collapsed and white, having little or no blood in them.

In order to try the effect of this air upon the blood itself, I took a quantity from a fowl just killed, and divided it into two parts, holding one of them within the

the fixed air, and the other in the common air, and observed that the former was coagulated much sooner than the latter. This I could wish to have tried again.

Insects and animals which breathe very little are stifled in fixed air, but are not soon quite killed in it. Butterflies, and flies of other kinds, will generally become torpid, and seemingly dead, after being held a few minutes over the fermenting liquor; but they revive again after being brought into the fresh air. But there are very great varieties with respect to the time in which different kinds of flies will either become torpid in the fixed air, or die in it. A large strong frog was much swelled, and seemed to be nearly dead, after being held about six minutes over the fermenting liquor; but it recovered upon being brought into the common air. A snail treated in the same manner died presently.

Fixed air is presently fatal to vegetable life. At least sprigs of mint, growing in water, and placed over the fermenting liquor, will often become quite dead in one day, or even in a less space of time; nor do they recover when they are afterwards brought into the common air. I am told, however, that some other plants are much more hardy in this respect.

A red rose, fresh gathered, lost its redness, and became of a purple colour, after being held over the fermenting liquor about twenty-four hours; but the tips of each leaf were much more affected than the rest of it. Another red rose turned perfectly white in this situation; but various other flowers, of different colours, were very little affected. These experiments

riments were not repeated, as I wish they might be done, in pure fixed air, extracted from chalk by means of oil of vitriol.

For every purpose, in which it was necessary that the fixed air should be as unmixed as possible, I generally made it by pouring oil of vitriol upon chalk and water, catching it in a bladder, fastened to the neck of the phial, in which they were contained, taking care to press out all the common air, and also the first, and sometimes the second, produce of fixed air; and also, by agitation, making it as quickly as I possibly could. At other times, I made it pass from the phial in which it was generated through a glass tube, without the intervention of any bladder, which, as I found by experience, will not long make a sufficient separation between several kinds of air and common air.

I had once thought that the readiest method of procuring fixed air, and in sufficient purity, would be by the simple process of burning chalk, or pounded lime-stone in a gun-barrel, making it pass through the stem of a tobacco-pipe, or a glass tube carefully luted to the orifice of it; and in this manner I find that air is produced in great plenty; but, upon examining it, I found, to my very great surprize, that little more than one half of it was fixed air, capable of being absorbed by water; and that the rest was inflammable, sometimes very weakly, but sometimes pretty highly so. Whence this inflammability proceeds, I am not able to determine, the lime or chalk not being supposed to contain any other than fixed air. I conjecture, however, that it must proceed from the iron, and the separation of it from

from the calx may be promoted by that small quantity of oil of vitriol, which I am informed is contained in chalk, if not in lime-stone also. But it is an objection to this hypothesis, that the inflammable air produced in this manner burns blue, and not at all like that which is produced from iron, or any other metal, by means of an acid. It has also the smell of that kind of inflammable air which is produced from vegetable substances. Besides, oil of vitriol without water, will not dissolve iron; nor can inflammable air be got from it, unless the acid be considerably diluted; and when I mixed brimstone with the chalk, neither the quality nor the quantity of the air was changed by it. Indeed no air, or permanently elastic vapour, can be got from brimstone, or any oil.

In the method in which I generally made the fixed air, and indeed always, unless the contrary be particularly mentioned, *viz.* by diluted oil of vitriol and chalk, I found by experiment that it was as pure as Mr. Cavendish made it. For after it had passed through a large body of water in small bubbles, still  $\frac{1}{4}$  or  $\frac{1}{5}$  part only was not absorbed by water. In order to try this as expeditiously as possible, I kept pouring the air from one glass vessel into another, immersed in a quantity of cold water, in which manner I found by experience, that almost any quantity may be reduced as far as possible in little more than a quarter of an hour.

At the same time that I was trying the purity of my fixed air, I had the curiosity to endeavour to ascertain whether that part of it which is not miscible in water, be equally diffused through the whole mass;

mass; and, for this purpose, I divided a quantity of about a gallon into three parts, the first consisting of that which was uppermost, and the last of that which was the lowest, contiguous to the water; but all these parts were reduced in about an equal proportion, by passing through the water, so that the whole mass had been of an uniform composition. This I have also found to be the case with several kinds of air, which will not properly incorporate.

A mouse will live very well, though a candle will not burn, in the residuum of the purest fixed air that I can make; and I once made a very large quantity for the sole purpose of this experiment. This, therefore, seems to be one instance of the generation of genuine common air, though vivified in some degree. It is also another proof of the residuum of fixed air being, in part at least, common air, that it becomes turbid, and is diminished by the mixture of nitrous air, as will be explained hereafter.

That fixed air only wants some addition to make it permanent, and immiscible with water, if not, in all respects, common air, I have been led to conclude, from several attempts which I once made to mix it with air, in which a quantity of iron filings and brimstone, made into a paste with water, had stood; for, in several mixtures of this kind, I imagined that not much more than half of the fixed air could be imbibed by water; but, not being able to repeat the experiment, I conclude that I either deceived myself in it, or that I overlooked some circumstance on which the success of it depended.

These experiments, however, whether they were fallacious or otherwise, induced me to try whether any

any alteration would be made in the constitution of fixed air, by this mixture of iron filings and brimstone. I therefore put a mixture of this kind into a quantity of as pure fixed air as I could make, and confined the whole in quicksilver, lest the water should absorbe it before the effects of the mixture could take place. The consequence was, that the fixed air was diminished, and the quicksilver rose in the vessel, till about the fifth part was occupied by it; and, as near as I could judge, the process went on, in all respects, as if the air in the inside had been common air.

What is most remarkable, in the result of this experiment, is, that the fixed air, into which this mixture had been put, and which had been in part diminished by it, was in part also rendered insoluble in water by this means. I made this experiment four times, with the greatest care, and observed, that in two of them about one sixth, and in the other two about one fourteenth, of the original quantity, was such as could not be absorbed by water, but continued permanently elastic. Lest I should have made any mistake with respect to the purity of the fixed air, the last time that I made the experiment, I set part of the fixed air, which I made use of, in a separate vessel, and found it to be exceedingly pure, so as to be almost wholly absorbed by water; whereas the other part, to which I had put the mixture, was far from being so.

In one of these cases, in which fixed air was made immiscible with water, it appeared to be not very noxious to animals; but in another case, a mouse died in it pretty soon.

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As the iron is reduced to a calx by this process, I once concluded, that it is phlogiston that fixed air wants, to make it common air; and, for any thing I yet know, this may be the case, though I am ignorant of the method of combining them; and when I calcined a quantity of lead in fixed air, in the manner which will be described hereafter, it did not seem to have been less soluble in water than it was before.

## II.

OF AIR IN WHICH A CANDLE, OR BRIMSTONE,  
HAS BURNED OUT.

It is well known that flame cannot subsist long without change of air, so that the common air is necessary to it, except in the case of substances, into the composition of which nitre enters; for these will burn *in vacuo*, in fixed air, and even under water, as is evident in some rockets, which are made for this purpose. The quantity of air which even a small flame requires to keep it burning is prodigious. It is generally said, that an ordinary candle consumes, as it is called, about a gallon in a minute. Considering this amazing consumption of air, by fires of all kinds, volcano's, &c. it becomes a great object of philosophical inquiry, to ascertain what change is made in the constitution of the air by flame, and to discover what provision there is in nature for remedying the injury which the atmosphere receives by this means. Some of the following experiments will, perhaps, be thought to throw a little light upon the subject.

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The diminution of the quantity of air in which a candle, or brimstone, has burned out, is various; but I imagine that, at a medium, it may be about one fifteenth, or one sixteenth, of the whole; about one third as much as by animals breathing it as long as they can, by animal or vegetable substances putrifying in it, by the calcination of metals, or by a mixture of steel filings and pounded brimstone standing in it.

I have sometimes thought, that flame disposes the common air to deposit the fixed air it contains; for if any lime-water be exposed to it, it immediately becomes turbid. This is the case, when wax candles, tallow candles, chips of wood, spirit of wine, æther, and every other substance which I have yet tried, except brimstone, is burned in a close glass vessel, standing in lime-water. This precipitation of fixed air (if this be the case) may be owing to something emitted from the burning bodies, which has a stronger affinity with the other constituent parts of the atmosphere.

If brimstone be burned in the same circumstances, the lime-water continues transparent, but still there may have been the same precipitation of the fixed part of the air; but that, uniting with the lime and the vitriolic acid, it forms a selenetic salt, which is soluble in water. Having evaporated a quantity of water thus impregnated, by burning brimstone a great number of times over it, a whitish powder remained, which had an acid taste; but repeating the experiment with a quicker evaporation, the powder had no acidity, but was very much like chalk. The burning of brimstone but once over a

quantity of lime-water, will affect it in such a manner, that breathing into it will not make it turbid, which otherwise it always presently does.

Dr. Hales supposed, that by burning brimstone repeatedly in the same quantity of air, the diminution would continue without end. But this I have frequently tried, and not found to be the case. Indeed, when the ignition has been imperfect in the first instance, a second firing of the same substance will increase the effect of the first, &c. but this progress soon ceases. In many cases of the diminution of air, the effect is not immediately apparent, even when it stands in water; for sometimes the bulk of air will not be much reduced, till it has passed several times through a quantity of water, which has thereby a better opportunity of absorbing that fluid part of the air, which had not been perfectly detached from the rest. I have sometimes found a very great reduction of a mass of air, in consequence of passing but once thorough cold water. If the air has stood in quicksilver, the diminution is generally inconsiderable, till it has undergone this operation, there not being any substance exposed to the air that could absorb any part of it.

I could not find any considerable alteration in the specific gravity of the air, in which candles, or brimstone, had burned out. I am satisfied, however, that it is not heavier than common air, which must have been manifest, if so great a diminution of the quantity had been owing, as Dr. Hales and others supposed, to the elasticity of the whole mass being impaired. After making several trials for this purpose, I concluded that air, thus diminished in bulk,  
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is rather lighter than common air, which favours the supposition of the fixed, or heavier part of the common air, having been precipitated.

An animal will live nearly, if not quite as long, in air in which candles have burned out, as in common air. This fact surprized me very greatly, having imagined that what is called the consumption of air by flame, or respiration, to have been of the same nature; but I have since found, that this fact has been observed by many persons, and even so early as by Mr. Boyle. I have also observed, that air in which brimstone has burned, is not in the least injurious to animals, after the fumes, which at first make it very cloudy, have intirely subsided.

Having read, in the Memoirs of the Society at Turin, Vol. I. p. 41. that air in which candles had burned out was perfectly restored, so that other candles would burn in it again as well as ever, after having been exposed to a considerable degree of cold, and likewise after having been compressed in bladders (for the cold had been supposed to have produced this effect by nothing but condensation): I repeated these experiments, and did, indeed, find, that, when I compressed the air in bladders, as the Count de Saluce, who made the observation, had done, the experiment succeeded: but having had sufficient reason to distrust bladders, I compressed the air in a glas vessel standing in water; and then I found, that this proceſs is altogether ineffectual for the purpose. I kept the air compressed much more, and much longer, than he had done, but without producing any alteration in it. I also find, that a greater degree of cold than that which he applied, and of

of longer continuance, did by no means restore this kind of air: for when I have exposed the phials which contained it a whole night, in which the frost was very intense; and also when I kept it surrounded with a mixture of snow and salt, I found it, in all respects, the same as before.

It is also advanced, in the same Memoir, p. 41. that heat only, as the reverse of cold, renders air unfit for candles burning in it. But I repeated the experiment of the Count for that purpose, without finding any such effect from it. I also remember that, many years ago, I filled an exhausted receiver with air, that had passed through a glass tube made red-hot, and found that a candle would burn in it perfectly well. Also, rarefaction by the air-pump does not injure air in the least degree.

Though this experiment failed, I flatter myself that I have accidentally hit upon a method of restoring air which has been injured by the burning of candles, and that I have discovered at least one of the restoratives which nature employs for this purpose. It is vegetation. In what manner this process in nature operates, to produce so remarkable an effect, I do not pretend to have discovered; but a number of facts declare in favour of this hypothesis. I shall introduce my account of them, by reciting some of the observations which I made on the growing of plants in confined air, which led to this discovery.

One might have imagined that, since common air is necessary to vegetable, as well as to animal life, both plants and animals had affected it in the same manner, and I own I had that expectation, when

when I first put a sprig of mint into a glass-jar, standing inverted in a vessel of water; but when it had continued growing there for some months, I found that the air would neither extinguish a candle, nor was it at all inconvenient to a mouse, which I put into it.

The plant was not affected any otherwise than was the necessary consequence of its confined situation; for plants growing in several other kinds of air, were all affected in the very same manner. Every succession of leaves was more diminished in size than the preceding, till, at length, they came to be no bigger than the heads of pins. The root decayed, and the stalk also, beginning from the root; and yet the plant continued to grow upwards, drawing its nourishment through a black and rotten stem. In the third or fourth set of leaves, long hairy filaments grew from the insertion of each leaf, and sometimes from the body of the stem, shooting out as far as the vessel in which it grew would permit, which, in my experiments, was about two inches. In this manner a sprig of mint lived, the old stem decaying, and new ones shooting up in its place, but less and less continually, all the summer season.

In repeating this experiment, care must be taken to draw away all the dead leaves from about the plant, lest they should putrefy, and affect the air. I have found that a fresh cabbage leaf, put under a glass vessel filled with common air, for the space of one night only, has so far affected the air, that a candle would not burn in it the next morning, and yet the leaf had not acquired any smell of putrefaction.

1.

Finding.

Finding that candles burn very well in air in which plants had grown a long time, and having had some reason to think, that there was something attending vegetation, which restored air that had been injured by respiration, I thought it was possible that the same process might also restore the air that had been injured by the burning of candles.

Accordingly, on the 17th of August, 1771, I put a sprig of mint into a quantity of air, in which a wax candle had burned out, and found that, on the 27th of the same month, another candle burned perfectly well in it. This experiment I repeated, without the least variation in the event, not less than eight or ten times in the remainder of the summer. Several times I divided the quantity of air in which the candle had burned out, into two parts, and putting the plant into one of them, left the other in the same exposure, contained, also, in a glass vessel immersed in water, but without any plant; and never failed to find, that a candle would burn in the former, but not in the latter. I generally found that five or six days were sufficient to restore this air, when the plant was in its vigour; whereas I have kept this kind of air in glass vessels, immersed in water many months, without being able to perceive that the least alteration had been made in it. I have also tried a great variety of experiments upon it, as by condensing, rarefying, exposing to the light and heat, &c. and throwing into it the effluvia of many different substances, but without any effect.

Experiments made in the year 1772, abundantly confirmed my conclusion concerning the restoration of air, in which candles had burned out by plants growing

growing in it. The first of these experiments was made in the month of May; and they were frequently repeated in that and the two following months, without a single failure.

For this purpose I used the flames of different substances, though I generally used wax or tallow candles. On the 24th of June the experiment succeeded perfectly well with air in which spirit of wine had burned out, and on the 27th of the same month it succeeded equally well with air in which brimstone matches had burned out, an effect of which I had despaired the preceding year.

This restoration of air I found depended upon the vegetating state of the plant; for though I kept a great number of the fresh leaves of mint in a small quantity of air in which candles had burned out, and changed them frequently, for a long space of time, I could perceive no melioration in the state of the air.

This remarkable effect does not depend upon any thing peculiar to mint, which was the plant that I always made use of till July 1772; for on the 16th of that month, I found a quantity of this kind of air to be perfectly restored by sprigs of balm, which had grown in it from the 7th of the same month.

That this restoration of air was not owing to any aromatic effluvia of these two plants, not only appeared by the essential oil of mint having no sensible effect of this kind; but from the equally complete restoration of this vitiated air by the plant called groundsel, which is usually ranked among the weeds, and has an offensive smell. This was the result of an experiment made the 16th of July, when the

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plant had been growing in the burned air from the 8th of the same month. Besides, the plant which I have found to be the most effectual of any that I have tried for this purpose is spinach, which is of quick growth, but will seldom thrive long in water. One jar of burned air was perfectly restored by this plant in four days, and another in two days. This last was observed on the 22d of July. In general this effect may be presumed to have taken place in much less time than I have mentioned; because I never chose to make a trial of the air, till I was pretty sure, from preceding observations, that the event which I had expected must have taken place, if it would succeed at all; lest, returning back that part of the air on which I made the trial, and which would thereby necessarily receive a small mixture of common air, the experiment might not be judged to be quite fair; though I myself might be sufficiently satisfied with respect to the allowance that was to be made for that small imperfection.

## III.

## OF INFLAMMABLE AIR.

I have generally made inflammable air in the manner described by Mr. Cavendish, in the Philosophical Transactions, from iron, zinc, or tin; but chiefly from the two former metals, on account of the process being the least troublesome: but when I extracted it from vegetable or animal substances, or from coals, I put them into a gun barrel, to the orifice of which I luted a glass tube, or the stem of

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a tobacco pipe, and to the end of this I tied a flaccid bladder, in order to catch the generated air.

There is not, I believe, any vegetable or animal substance whatever, nor any mineral substance, that is inflammable, but what will yield great plenty of inflammable air, when they are treated in this manner, and urged with a strong heat; but, in order to get the most air, the heat must be applied as suddenly, and as vehemently, as possible. For, notwithstanding the same care be taken in luting, and in every other respect, six or even ten times more air may be got by a sudden heat than by a slow one, though the heat that be last applied be as intense as that which was applied suddenly. A bit of dry oak, weighing about twelve grains, will generally yield about a sheep's bladder full of inflammable air with a brisk heat, when it will only give about two or three ounce measures if the same heat be applied to it very gradually. To what this difference is owing, I cannot tell.

Inflammable air, when it is made by a quick process, has a very strong and offensive smell, from whatever substance it be generated; but this smell is of three different kinds, according as the air is extracted from mineral, vegetable, or animal substances. The last is exceedingly fetid; and it makes no difference, whether it be extracted from a bone, or even an old and dry tooth, or from soft muscular flesh, or any other part of the animal. The burning of any substance occasions the same smell: for the gross fume which arises from them, before they flame, is the inflammable air they contain, which is expelled by heat, and then readily ignited. The smell of inflammable

flammable air is the very same, as far as I am able to perceive, from whatever substance of the same kingdom it be extracted. Thus it makes no difference whether it be got from iron, zinc, or tin, from any kind of wood, or, as was observed before, from any part of an animal.

If a quantity of inflammable air be contained in a glass vessel standing in water, and have been generated very fast, it will smell even through the water, and this water will also soon become covered with a thin film, assuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red okre, or the earth of iron, as I have found by collecting a considerable quantity of it; and if it have been generated from zinc, it is a whitish substance, which I suppose to be the calx of the metal. It likewise settles to the bottom of the vessel, and when the water is stirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this scum for a considerable time after the air is removed from it. This I have often observed with respect to iron.

Inflammable air, made by a violent effervescence, I have observed to be much more inflammable than that which is made by a weak effervescence, whether the water or the oil of vitriol prevailed in the mixture. Also the offensive smell was much stronger in the former case than in the latter. The greater degree of inflammability appeared by the greater number of successive explosions, when a candle was presented to the neck of a phial filled with it. It is possible, however, that this diminution of inflammability

flammability may, in some measure, arise from the air continuing so much longer in the bladder when it is made very slowly; though I think the difference is too great for this cause to have produced the whole of it. It may, perhaps, deserve to be tried by a different process, without a bladder.

Inflammable air is not thought to be miscible with water, and when kept many months, seems, in general, to be as inflammable as ever. Indeed, when it is extracted from vegetable or animal substances, a part of it will be imbibed by the water in which it stands; but it may be presumed, that in this case, there was a mixture of fixed air extracted from the substance along with it. I have indisputable evidence, however, that inflammable air, standing long in water, has actually lost all its inflammability, and even come to extinguish flame much more than that air in which candles have burned out. After this change it appears to be greatly diminished in quantity, and it still continues to kill animals the moment they are put into it.

This very remarkable fact first occurred to my observation on the twenty fifth of May 1771, when I was examining a quantity of inflammable air, which had been made from zinc, near three years before. Upon this, I immediately set by a common quart bottle filled with inflammable air from iron, and another equal quantity from zinc; and examining them in the beginning of December following, that from the iron was reduced near one half in quantity, if I be not greatly mistaken; for I found the bottle half full of water, and I am pretty clear that it was full of air when it was set by. That which had been

been produced from zinc was not altered, and filled the bottle as at first.

Another instance of this kind occurred to my observation on the 19th of June 1772, when a quantity of air, half of which had been inflammable air from zinc, and half air in which mice had died, and which had been put together the 30th of July 1771, appeared not to be in the least inflammable, but extinguished flame, as much as any kind of air that I had ever tried. I think that, in all, I have had four instances of inflammable air losing its inflammability, while it stood in water.

Though air tainted with putrefaction extinguishes flame, I have not found that animals or vegetables putrefying in inflammable air render it less inflammable. But one quantity of inflammable air, which I had set by in May 1771, along with the others above mentioned, had had some putrid flesh in it; and this air had lost its inflammability, when it was examined at the same time with the other in the December following. The bottle in which this air had been kept, smelled exactly like very strong Harrowgate water. I do not think that any person could have distinguished them.

I have made plants grow for several months in inflammable air made from zinc, and also from oak; but, though the plants grew pretty well, the air still continued inflammable. The former, indeed, was not so highly inflammable as when it was fresh made, but the latter was quite as much so; and the diminution of inflammability in the former case, I attribute to some other cause than the growth of the plant.

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No kind of air, on which I have yet made the experiment, will conduct electricity; but the colour of a spark is remarkably different in some different kinds of air, which seems to shew that they are not equally good non-conductors. In fixed air, the electric spark is exceedingly white; but in inflammable air it is of a purple, or red colour. Now, since the most vigorous sparks are always the whitest, and, in other cases, when the spark is red, there is reason to think that the electric matter passes with difficulty, and with less rapidity: it is possible that the inflammable air may contain particles which conduct electricity, though very imperfectly; and that the whiteness of the spark in the fixed air, may be owing to its meeting with no conducting particles at all. When an explosion was made in a quantity of inflammable air, it was a little white in the center, but the edges of it were still tinged with a beautiful purple. The degree of whiteness in this case was probably owing to the electric matter rushing with more violence in an explosion than in a common spark.

Inflammable air kills animals as suddenly as fixed air, and, as far as can be perceived, in the same manner, throwing them into convulsions, and thereby occasioning present death. I had imagined that, by animals dying in a quantity of inflammable air, it would in time become less noxious; but this did not appear to be the case; for I killed a great number of mice in a small quantity of this air, which I kept several months for this purpose, without its being at all sensibly mended; the last, as well as the first mouse, dying the moment it was put into it.

I once imagined that, since fixed and inflammable air are the reverse of one another, in several remarkable properties, a mixture of them would make common air; and while I made the mixtures in bladders, I imagined that I had succeeded in my attempt; but I have since found that thin bladders do not sufficiently prevent the air that is contained in them from mixing with the external air. Also corks will not sufficiently confine different kinds of air, unless the phials in which they are confined be set with their mouths downwards, and a little water lie in the necks of them, which, indeed, is equivalent to the air standing in vessels immersed in water. In this manner, however, I have kept different kinds of air for several years.

Whatever methods I took to promote the mixture of fixed and inflammable air, they were all ineffectual. I think it my duty, however, to recite the issue of an experiment or two of this kind, in which equal mixtures of these two kinds of air had stood near three years, as they seem to shew that they had in part affected one another, in that long space of time. These mixtures I examined April 27, 1771. One of them had stood in quicksilver, and the other in a corked phial, with a little water in it. On opening the latter in water, the water instantly rushed in, and filled almost half of the phial, and very little more was absorbed afterwards. In this case the water in the phial had probably absorbed a considerable part of the fixed air, so that the inflammable air was exceedingly rarefied; and yet the whole quantity that must have been rendered non-elastic was ten times more than the bulk of the water, and it has not

not been found that water can contain much more than its own bulk of fixed air. But in other cases I have found the diminution of a quantity of air, and especially of fixed air, to be much greater than I could well account for by any kind of absorption.

The phial which had stood immersed in quicksilver had lost very little of its original quantity; and being now opened in water, and left there, along with a another phial, which was just then filled, as this had been three years before, with air half inflammable and half fixed, I observed that the quantity of both was diminished, by the absorption of the water, in the same proportion.

Upon applying a candle to the mouths of the phials which had been kept three years, that which had stood in quicksilver went off at one explosion, exactly as it would have done if there had been a mixture of common air, with the inflammable. As a good deal depends upon the apertures of the vessels in which the inflammable air is fixed, I mixed the two kinds of air in equal proportion in the same phial, and after letting it stand some days in water, that the fixed air might be absorbed, I applied a candle to it; but it made ten or twelve explosions (stopping the phial after each of them) before the inflammable matter was exhausted.

The air which had been confined in the corked phial exploded in the very same manner as an equal mixture of the two kinds of air in the same phial, the experiment being made as soon as the fixed air was absorbed, as before; so that, in this case, the two kinds of air did not seem to have affected one another at all.

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Considering inflammable air as air united to or loaded with phlogiston, I exposed to it several substances, which are said to have a near affinity with phlogiston, as oil of vitriol, and spirit of nitre (the former for above a month), but without making any sensible alteration in it.

I observed, however, that inflammable air, mixed with the fumes of smoaking spirit of nitre, goes off at one explosion, exactly like a mixture of half common and half inflammable air. This I tried several times, by throwing the inflammable air into a phial full of spirit of nitre, with its mouth immersed in a basin containing some of the same spirit, and then applying the flame of a candle to the mouth of the phial, the moment that it was uncovered, after it had been taken out of the basin. This remarkable effect I hastily concluded to have arisen from the inflammable air having been in part deprived of its inflammability, by means of the stronger affinity, which the spirit of nitre had with phlogiston, and therefore I imagined that by letting them stand longer in contact, and especially by agitating them strongly together, I should deprive the air of all its inflammability; but neither of these operations succeeded, for still the air was only exploded at once, as before. And lastly, when I passed a quantity of inflammable air, which had been mixed with the fumes of spirit of nitre, through a body of water, and received it in another vessel, it appeared not to have undergone any change at all, for it went off in several successive explosions, like the purest inflammable air. The effect abovementioned must, therefore, have been owing to the fumes of the spirit of nitre supplying the

the place of common air for the purpose of ignition, which is analogous to other experiments with nitre.

Having had the curiosity, on the 25th of July, 1772, to expose a great variety of different kinds of air to water out of which the air it contained had been boiled, without any particular view; the result was, in several respects, altogether unexpected, and led to a variety of new observations on the properties and affinities of several kinds of air with respect to water. Among the rest three fourths of that which was inflammable was absorbed by the water in about two days, and the remainder was inflammable, but weakly so.

Upon this, I began to agitate a quantity of strong inflammable air in a glass jar, standing in a pretty large trough of water, the surface of which was exposed to the common air, and I found that when I had continued the operation about ten minutes, near one fourth of the quantity of air had disappeared; and finding that the remainder made an effervescence with nitrous air, I concluded that it must have become fit for respiration, whereas this kind of air is, at the first, as noxious as any other kind whatever. To ascertain this, I put a mouse into a vessel containing  $2\frac{1}{2}$  ounce measures of it, and observed that it lived in it twenty minutes, which is as long as a mouse will generally live in the same quantity of common air. This mouse was even taken out alive, and recovered very well. Still also the air in which it had breathed so long was inflammable, though very weakly so. I have even found it to be so when a mouse has actually died in it.

Inflammable air thus diminished by agitation in water, makes but one explosion on the approach of a candle exactly like a mixture of inflammable air with common air.

From this experiment I concluded that, by continuing the same process, I should deprive inflammable air of all its inflammability, and this I found to be the case; for, after a longer agitation, it admitted a candle to burn in it, like common air, only more faintly; and indeed by the test of nitrous air it did not appear to be near so good as common air. Continuing the same process still farther, the air which had been most strongly inflammable a little before, came to extinguish candle, exactly like air in which a candle had burned out, nor could they be distinguished by the test of nitrous air.

I found, by repeated trials, that it was difficult to catch the time in which inflammable air obtained from metals, in coming to extinguish flame, was in the state of common air, so that the transition from the one to the other must be very short. I readily, however, found this state in a quantity of inflammable air extracted from oak, which air I had kept by me a year, and in which a plant had grown, though very poorly, for some part of the time. A quantity of this air, after being agitated in water till it was diminished about one half, admitted a candle to burn in it exceedingly well, and was even hardly to be distinguished from common air by the test of nitrous air.

I took some pains to ascertain the quantity of diminution, in fresh made and very highly inflammable air from iron, at which it ceased to be inflammable,

mable, and, upon the whole, I concluded that it was so when it was diminished a little more than one half : for a quantity which was diminished exactly one half had something inflammable in it, but in the slightest degree imaginable.

Finding that water would imbibe inflammable air, I endeavoured to impregnate water with it, by the same process by which I had made water imbibe fixed air ; but though I found that distilled water would imbibe about one fourteenth of its bulk of inflammable air, I could not perceive that the taste of it was sensibly altered.

#### IV.

##### OF AIR INFECTED WITH ANIMAL RESPIRATION, OR PUTREFACTION.

That candles will burn only a certain time, is a fact not better known, than it is that animals can live only a certain time, in a given quantity of air ; but the cause of the death of the animal is not better known than that of the extinction of flame in the same circumstances ; and when once any quantity of air has been rendered noxious by animals breathing in it as long as they could, I do not know that any methods have been discovered of rendering it fit for breathing again. It is evident, however, that there must be some provision in nature for this purpose, as well as for that of rendering the air fit for sustaining flame ; for without it the whole mass of the atmosphere would, in time, become unfit for the purpose of animal life ; and yet there is no reason to think that it is, at present, at all less fit for respiration than it

it has ever been. I flatter myself, however, that I have hit upon two of the methods employed by nature for this great purpose. How many others there may be, I cannot tell.

When animals die upon being put into air in which other animals have died, after breathing in it as long as they could, it is plain that the cause of their death is not the want of any *pabulum vitæ*, which has been supposed to be contained in the air, but on account of the air being impregnated with something stimulating to their lungs; for they almost always die in convulsions, and are sometimes affected so suddenly, that they are irrecoverable after a single inspiration, though they be withdrawn immediately, and every method has been taken to bring them to life again. They are affected in the same manner, when they are killed in any other kind of noxious air that I have tried, viz. fixed air, inflammable air, air filled with the fumes of brimstone, infected with putrid matter, in which a mixture of iron filings and brimstone has stood, or in which charcoal has been burned, or metals calcined, or in nitrous air, &c.

If a mouse (which is an animal that I have commonly made use of for the purpose of these experiments) can stand the first shock of this stimulus, or has been habituated to it by degrees, it will live a considerable time in air in which other mice will die instantaneously. I have frequently found that when a number of mice have been confined in a given quantity of air, less than half the time that they have actually lived in it, a fresh mouse has been instantly thrown into convulsions, and died upon being put to them. It is evident, therefore, that if the

the experiment of the Black Hole were to be repeated, a man would stand the better chance of surviving it, who should enter at the first, than at the last hour. I have also observed, that young mice will always live much longer than old ones, or than those which are full grown, when they are confined in the same quantity of air. I have sometimes known a young mouse to live six hours in the same circumstances in which an old mouse has not lived one. On these accounts, experiments with mice, and, for the same reason, no doubt, with other animals also, have a considerable degree of uncertainty attending them; and therefore, it is necessary to repeat them frequently, before the result can be absolutely depended upon.

The discovery of the provision in nature for restoring air, which has been injured by the respiration of animals, having long appeared to me to be one of the most important problems in natural philosophy, I have tried a great variety of schemes in order to effect it. In these, my guide has generally been to consider the influences to which the atmosphere is, in fact, exposed; and, as some of my unsuccessful trials may be of use to those who are disposed to take pains in the farther investigation of this subject, I shall mention the principal of them.

The noxious effluvium with which air is loaded by animal respiration, is not absorbed by standing without agitation in fresh or salt water. I have kept it many months in fresh water, when, instead of being meliorated, it has seemed to become even more deadly, so as to require more time to restore it, by the methods which will be explained hereafter, than air

air which has been lately made noxious. I have even spent several hours in pouring this air from one glass vessel into another, in water, sometimes as cold, and sometimes as warm, as my hands could bear it, and have sometimes also wiped the vessels many times, during the course of the experiment, in order to take off that part of the noxious matter, which might adhere to the glass vessels, and which evidently gave them an offensive smell; but all these methods were generally without any sensible effect. The motion, also, which the air received in these circumstances; it is very evident, was of no use for this purpose.

This kind of air is not restored by being exposed to the light, or by any other influence to which it is exposed, when confined in a thin phial, in the open air, for some months.

Among other experiments, I tried a great variety of different effluvia, which are continually exhaling into the air, especially of those substances which are known to resist putrefaction; but I could not by these means effect any melioration of the noxious quality of this kind of air.

Having read, in the Memoirs of the Imperial Society, of a plague not afflicting a particular village, in which there was a large sulphur work, I immediately fumigated a quantity of this kind of air; or (which will hereafter appear to be the very same thing) air tainted with putrefaction, with the fumes of burning brimstone, but without any effect.

I once imagined, that the nitrous acid in the air might be the general restorative which I was in quest of; and the conjecture was favoured, by finding

ing that candles would burn, and animals live, in air extracted from saltpetre. I therefore spent a good deal of time in attempting, by a burning-glass, and other means, to impregnate this noxious air with some effluvium of saltpetre, and, with the same view, introduced into it the fumes of the smoaking spirit of nitre; but both these methods were altogether ineffectual.

In order to try the effect of heat, I put a quantity of air, in which mice had died, into a bladder, tied to the end of the stem of a tobacco-pipe, at the other end of which was another bladder, out of which the air was carefully pressed. I then put the middle part of the stem into a chafing-dish of hot coals, strongly urged with a pair of bellows; and, pressing the bladders alternately, I made the air pass several times through the heated part of the pipe. I have also made this kind of air very hot, standing in water before the fire. But neither of these methods were of any use.

Rarefaction and condensation by instruments were also tried, but in vain.

Thinking it possible that the earth might imbibe the noxious quality of the air, and thence supply the roots of plants with such putrescent matter as is known to be nutritive to them, I kept a quantity of air, in which mice had died, in a phial, one half of which was filled with fine garden mould; but, though it stood two months in these circumstances, it was not the better for it.

I once imagined that, since several kinds of air cannot be long separated from common air, by being confined in bladders, in bottles well corked, or even

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closed with ground stopples, the affinity between this noxious air and the common air might be so great, that they would mix through a body of water interposed between them; the water continually receiving from the one, and giving to the other, especially as water receives some kinds of impregnation from, I believe, every kind of air to which it is contiguous; but I have seen no reason to conclude, that a mixture of any kind of air with the common air can be produced in this manner. I have kept air in which mice have died, air in which candles have burned out, and inflammable air, separated from the common air, by the slightest partition of water that I could well make, so that it might not evaporate in a day or two, if I should happen not to attend to them; but I found no change in them after a month or six weeks. The inflammable air was still inflammable, mice died instantly in the air in which other mice had died before, and candles would not burn where they had burned out before.

Since air tainted with animal or vegetable putrefaction is the same thing with air rendered noxious by animal respiration, I shall now recite the observations which I have made upon this kind of air, before I treat of the method of restoring them.

That these two kinds of air are, in fact, the same thing, I conclude from their having several remarkable common properties, and from their differing in nothing that I have been able to observe. They equally extinguish flame, they are equally noxious to animals, they are equally, and in the same way, offensive to the smell, they are equally diminished in

in their quantity, they equally precipitate in lime-water, and they are restored by the same means.

Since air which has passed through the lungs is the same thing with air tainted with animal putrefaction, it is probable that one use of the lungs is to carry off a putrid effluvium, without which, perhaps, a living body might putrefy as soon as a dead one.

When a mouse putrefies in any given quantity of air, the bulk of it is generally increased for a few days; but in a few days more it begins to shrink up, and generally, in about eight or ten days, if the weather be pretty warm, it will be found to be diminished  $\frac{1}{2}$ , or  $\frac{1}{3}$  of its bulk. If it do not appear to be diminished after this time, it only requires to be passed through water, and the diminution will not fail to be sensible. I have sometimes known almost the whole diminution to take place, upon once or twice passing through the water. The same is the case with air, in which animals have breathed as long as they could. Also, air in which candles have burned out may almost always be farther reduced by this means. All these processes, as I observed before, seem to dispose the compound mass of air to part with some constituent part belonging to it; and this being miscible with water, must be brought into contact with it, in order to mix with it to the most advantage, especially when its union with the other constituent principles of the air is but partially broken.

I have put mice into vessels which had their mouths immersed in quicksilver, and observed that the air was not much contracted after they were dead or cold; but upon withdrawing the mice, and admitting

lime-water to the air it immediately became turbid, and was contracted in its dimensions as usual.

I tried the same thing with air tainted with putrefaction, putting a dead mouse to a quantity of common air, in a vessel which had its mouth immersed in quicksilver, and after a week I took the mouse out, drawing it through the quicksilver, and observed that for some time there was an apparent increase of the air perhaps about  $\frac{1}{10}$ . After this, it stood two days in the quicksilver, without any sensible alteration; and then admitting water to it, it began to be absorbed, and continued so, till the original quantity was diminished about  $\frac{1}{2}$ . If, instead of common water, I had made use of lime water in this experiment, I make no doubt but it would have become turbid.

If a quantity of lime-water in a phial be put under a glass vessel standing in water, it will not become turbid, and provided the access of the common air be prevented, it will continue lime-water, I do not know how long; but if a mouse be left to putrefy in the vessel, the water will deposit all its lime in a few days. This may be owing to the fixed air being transferred from the putrid mouse into the water, and yet it is evident that there is a putrid effluvium entirely distinct from this kind of air, and which has very different properties.

It is a doubt with me, however, whether the putrid effluvium be not chiefly fixed air, with the addition of some other effluvium, which has the power of diminishing common air. The resemblance between the true putrid effluvium and fixed air in the following experiment, which is as decisive

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as I can possibly contrive it, appeared to be very great; indeed, much greater than I had expected. I put a dead mouse into a tall glass vessel, and having filled the remainder with quicksilver, and set it, inverted, in a pot of quicksilver, I let it stand about two months, in which time the putrid effluvium issuing from the mouse had filled the whole vessel, and part of the dissolved blood, which lodged upon the surface of the quiksilver, began to be thrown out. I then filled another glass vessel, of the same size and shape, with as pure fixed air as I could make, and exposed them both, at the same time, to a quantity of lime-water. In both cases the water grew turbid alike, it rose equally fast in both the vessels, and likewise equally high; so that about the same quantity remained unabſorbed by the water. One of these kinds of air, however, was exceedingly sweet and pleasant, and the other insufferably offensive; one of them also would have made an addition to any quantity of common air with which it had been mixed, and the other would have diminished it. This, at least, would have been the consequence, if the mouse itself had putrefied in any quantity of air.

It seems to depend, in some measure, upon the time, and other circumstances, in the dissolution of animal or vegetable substances, whether they yield the proper putrid effluvium, or fixed, or inflammable air; but the experiments which I have made upon this subject, have not been numerous enough to enable me to decide with certainty concerning those circumstances. Putrid cabbage, green, or boiled, infects the air in the very same manner as putrid animal substances. Air thus tainted is equally contracted in

in its dimensions, it equally extinguishes flame, and is equally noxious to animals; but they affect the air very differently if the heat that is applied to them be considerable. If beef or mutton, raw, or boiled, be placed so near to the fire, that the heat to which it is exposed shall equal, or rather exceed, that of the blood, a considerable quantity of air will be generated in a day or two, about  $\frac{1}{4}$ th of which I have generally found to be absorbed by water, while all the rest was inflammable; but air generated from vegetables, in the same circumstances, will be almost all fixed, and no part of it inflammable. This I have repeated again and again, the whole process being in quicksilver; so that neither common air, nor water, had any access to the substance on which the experiment was made; and the generation of air, or effluvium of any kind, except what might be absorbed by quicksilver, or resorbed by the substance itself, might be distinctly noted.

A vegetable substance, after standing a day or two in these circumstances, will yield nearly all the air that can be extracted from it, in that degree of heat; whereas an animal substance will continue to give more air or effluvium, of some kind or other, with very little alteration, for many weeks. It is remarkable, however, that though a piece of beef or mutton, plunged in quicksilver, and kept in this degree of heat, yield air, the bulk of which is inflammable, and contracts no putrid smell (at least, in a day or two), a mouse treated in the same manner, yields the proper putrid effluvium, as, indeed the smell sufficiently indicates; and this effluvium does either

either itself extinguish flame, or has in it such a mixture of fixed air, as to give it that property.

That the putrid effluvium will mix with water seems to be evident from the following experiment. If a mouse be put into a jar full of water, standing with its mouth inverted in another vessel of water, a considerable quantity of elastic matter (and which may, therefore, be called air) will soon be generated, unless the weather be so cold as to check all putrefaction. After a short time, the water contracts an extremely fetid and offensive smell, which seems to indicate that the putrid effluvium pervades the water, and affects the neighbouring air; and since, after this, there is often no increase of the air, that seems to be the very substance which is carried off through the water, as fast as it is generated; and the offensive smell is a sufficient proof that it is not fixed air. For this has a very agreeable flavour, whether it be produced by fermentation, or extracted from chalk by oil of vitriol; affecting not only the mouth, but even the nostrils, with a pungency which is peculiarly pleasing to a certain degree, as any person may easily satisfy himself who will chuse to make the experiment. If the water in which the mouse was immersed, and which is saturated with the putrid air, be changed, the greater part of the putrid air will, in a day or two, be absorbed, though the mouse continues to yield the putrid effluvium as before; for as soon as this fresh water becomes saturated with it, it begins to be offensive to the smell, and the quantity of the putrid air upon its surface increases as before. I kept a mouse producing putrid air in this manner for the space of several months..

Six ounce measures of air not readily absorbed by water, appeared to have been generated from one mouse, which had been putrefying eleven days in confined air, before it was put into a jar which was quite filled with water, for the purpose of this observation.

Air thus generated from putrid mice standing in water, without any mixture of common air, extinguishes flame, and is noxious to animals, but not more so than common air only tainted with putrefaction. It is exceedingly difficult and tedious to collect a quantity of this putrid air, not miscible in water, so very great a proportion of what is collected being absorbed by the water, in which it is kept; but what that proportion is, I have not endeavoured to ascertain.

Though a quantity of air be diminished by any substance putrefying in it, I have not yet found the same effect to be produced by a mixture of putrid air with common air; but, in the manner in which I have hitherto made the experiment, I was obliged to let the putrid air, pass through a body of water; which might instantly absorb whatever it was in the putrid substance, that diminished the common air.

Insects of various kinds live perfectly well in air tainted with animal or vegetable putrefaction, when a single inspiration of it would have instantly killed any animal. I have frequently tried the experiment with flies and butterflies. I have also observed, that the *aphides* will thrive as well upon plants growing in this kind of air, as in the open air. I have even been frequently obliged to take plants out of the putrid air in which they were growing, on purpose to brush away the swarms of

these insects which infected them; and yet so effectually did some of them conceal themselves; and so fast did they multiply, in these circumstances, that I could seldom keep the plants quite clear of them.

When air has been freshly and strongly tainted with putrefaction, so as to smell through the water, sprigs of mint have presently died, upon being put into it, their leaves turning black; but if they do not die presently, they thrive in a most surprizing manner. In no other circumstances have I ever seen vegetation so vigorous as in this kind of air, which is immediately fatal to animal life. Though these plants have been crowded in jars filled with this air, every leaf has been full of life; fresh shoots have branched out in various directions, and have grown much faster than other similar plants, growing in the same exposure in common air.

This observation led me to conclude, that plants, instead of affecting the air in the same manner with animal respiration, reverse the effects of breathing, and tend to keep the atmosphere sweet and wholesome, when it is become noxious, in consequence of animals living and breathing, or dying and putrefying in it.

In order to ascertain this, I took a quantity of air, made thoroughly noxious, by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immersed in water; and to the other (which was contained in a glass jar, standing in water) I put a sprig of mint. This was about the beginning of August 1771, and after eight or nine days, I found that a mouse lived perfectly well

in that part of the air, in which the sprig of mint had grown, but died the moment it was put into the other part of the same original quantity of air; and which I had kept in the very same exposure, but without any plant growing in it.

This experiment I have several times repeated; sometimes using air, in which animals had breathed and died; sometimes using air tainted with vegetable or animal putrefaction, and generally with the same success.

Once, I let a mouse live and die in a quantity of air, which had been noxious, but which had been restored by this process, and it lived nearly as long as I conjectured it might have done in an equal quantity of fresh air; but, this is so exceedingly various, that it is not easy to form any judgment from it; and in this case the symptom of *difficult respiration* seemed to begin earlier than it would have done in common air.

Since the plants that I made use of manifestly grow and thrive in putrid air; since putrid matter is well known to afford proper nourishment for the roots of plants; and since it is likewise certain that they receive nourishment by their leaves as well as by their roots, it seems to be exceedingly probable, that the putrid effluvium is in some measure extracted from the air, by means of the leaves of plants, and therefore that they render the remainder more fit for respiration.

Towards the end of the year some experiments of this kind did not answer so well as they had done before, and I had instances of the relapsing of this restored air to its former noxious state. I therefore suspended

suspended my judgment concerning the efficacy of plants to restore this kind of noxious air, till I should have an opportunity of repeating my experiments, and giving more attention to them. Accordingly I resumed the experiments in the summer of the year 1772, when I presently had the most indisputable proof of the restoration of putrid air by vegetation; and as the fact is of some importance, and the subsequent variation in the state of this kind of air is a little remarkable; I think it necessary to relate some of the facts pretty circumstantially.

The air, on which I made the first experiments, was rendered exceedingly noxious by mice dying in it on the 20th of June. Into a jar nearly filled with one part of this air, I put a sprig of mint, while I kept another part of it in a phial, in the same exposure; and on the 27th of the same month, and not before, I made a trial of it, by introducing a mouse into a glass vessel, containing 2½ ounce measures filled with each kind of air; and I noted the following facts.

When the vessel was filled with the air in which the mint had grown, a very large mouse lived five minutes in it, before it began to shew any sign of uneasiness. I then took it out, and found it to be as strong and vigorous as when it was first put in; whereas in that air which had been kept in the phial only, without a plant growing in it, a younger mouse continued not longer than two or three seconds, and was taken out quite dead. It never breathed after, and was immediately motionless. After half an hour, in which time the larger mouse

(which I had kept alive, that the experiment might be made on both the kinds of air with the very same animal) would have been sufficiently recruited, supposing it to have received any injury by the former experiment, was put into the same vessel of air ; but though it was withdrawn again, after being in it hardly one second, it was recovered with difficulty, not being able to stir from the place for near a minute. After two days, I put the same mouse into an equal quantity of common air, and observed that it continued seven minutes without any sign of uneasiness ; and being very uneasy after three minutes longer, I took it out. Upon the whole, I concluded that the restored air wanted about one fourth of being as wholesome as common air. The same thing also appeared when I applied the test of nitrous air.

In the seven days, in which the mint was growing in this jar of noxious air, three old shoots had extended themselves about three inches, and several new ones had made their appearance, in the same time. Dr. Franklin and Sir John Pringle happened to be with me, when the plant had been three or four days in this state, and took notice of its vigorous vegetation, and remarkably healthy appearance, in that confinement.

On the 30th of the same month, a mouse lived fourteen minutes, breathing naturally all the time, and without appearing to be much uneasy, till the last two minutes, in air which had been rendered noxious by mice breathing in it almost a year before, and which I had found to be most highly noxious on the 19th of this month, a plant having grown in it, but

but not exceedingly well, these eleven days; on which account, I had deferred making the trial so long. This restored air was affected by a mixture of nitrous air, almost as much as common air.

As this putrid air was thus easily restored to a considerable degree of fitness for respiration, by plants growing in it, I was in hopes that by the same means it might in time be so much more perfectly restored, that a candle would burn in it; and for this purpose I kept plants growing in the jars which contained this air till the middle of August following, but did not take sufficient care to pull out all the old and rotten leaves. The plants, however, had grown, and looked so well upon the whole, that I had no doubt but that the air must constantly have been in a mending state; when I was exceedingly surprized to find, on the 24th of that month, that though the air in one of the jars had not grown worse, it was no better, and that the air in the other jar was so much worse than it had been, that a mouse would have died in it in a few seconds. It also made no effervescence with nitrous air, as it had done before.

Suspecting that the same plant might be capable of restoring putrid air to a certain degree only, or that plants might have a contrary tendency in some stages of their growth, I withdrew the old plant, and put a fresh one in its place; and found that, after seven days, the air was restored to its former wholesome state. This fact I consider as a very remarkable one, and well deserving of a farther investigation, as it may throw more light upon the principles of vegetation. It is not, however,

a single fact ; for I had several instances of the same kind in the preceding year ; but it seemed so very extraordinary, that air should grow worse by the continuance of the same treatment by which it had grown better, that, whenever I observed it, I concluded that I had not taken sufficient care to satisfy myself of its previous restoration.

That plants are capable of perfectly restoring air injured by respiration, may, I think, be inferred with certainty from the perfect restoration, by this means, of air which had passed through my lungs, so that a candle would burn in it again, though it had extinguished flame before, and a part of the same original quantity of air still continued to do so. Of this one instance occurred in the year 1771, a sprig of mint having grown in a jar of this kind of air, from the 25th of July to the 17th of August following ; and another trial I made with the same success the 7th of July 1772, the plant having grown in it from the 29th of June preceding. In this case also I found that the effect was not owing to any virtue in the leaves of mint ; for I kept them constantly changed in a quantity of this kind of air, for a considerable time, without making any sensible alteration in it.

These proofs of a partial restoration of air by plants in a state of vegetation, though in a confined and unnatural situation, cannot but render it highly probable, that the injury which is continually done to the atmosphere by the respiration of such a number of animals, and the putrefaction of such masses of both vegetable and animal matter, is, in part at least, repaired by the vegetable creation.

And,

And, notwithstanding the prodigious mass of air that is corrupted daily by the abovementioned causes; yet, if we consider the immense profusion of vegetables upon the face of the earth, growing in places suited to their nature, and consequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought, but that it may be a sufficient counterbalance to it, and that the remedy is adequate to the evil.

Dr. Franklin, who, as I have already observed, saw some of my plants in a very flourishing state, in highly noxious air, was pleased to express very great satisfaction with the result of the experiments. In his answer to the letter in which I informed him of it, he says,

“ That the vegetable creation should restore the air which is spoiled by the animal part of it, looks like a rational system, and seems to be of a piece with the rest. Thus fire purifies water all the world over. It purifies it by distillation, when it raises it in vapours, and lets it fall in rain; and farther still by filtration, when, keeping it fluid, it suffers that rain to percolate the earth. We knew before, that putrid animal substances were converted into sweet vegetables, when mixed with the earth, and applied as manure; and now, it seems, that the same putrid substances, mixed with the air, have a similar effect. The strong thriving state of your mint in putrid air seems to shew that the air is mended by taking something from it, and not by adding to it.” He adds, “ I hope this will give some check to the rage of destroying trees

" that grow near houses, which has accompanied  
 " our late improvements in gardening, from an  
 " opinion of their being unwholesome. I am cer-  
 " tain, from long observation, that there is no-  
 " thing unhealthy in the air of woods; for we  
 " Americans have every where our country habi-  
 " tations in the midst of woods, and no people on  
 " earth enjoy better health, or are more prolific."

Having rendered inflammable air perfectly in-  
 noxious by continued agitation in a trough of water,  
 deprived of its air, I concluded that other kinds of  
 noxious air might be restored by the same means;  
 and I presently found that this was the case with  
 putrid air, even of more than a year's standing. I  
 shall observe once for all, that this process has ne-  
 ver failed to restore any kind of noxious air on  
 which I have tried it, viz. air injured by respiration  
 or putrefaction, air infected with the fumes  
 of burning charcoal, and of calcined metals, air  
 in which a mixture of iron filings and brimstone,  
 or that in which paint made of white lead and oil  
 has stood, or air which has been diminished by a  
 mixture of nitrous air. Of the remarkable effect  
 which this process has on nitrous air itself, an ac-  
 count will be given in its proper place.

If this process be made in water deprived of air,  
 either by the air pump, by boiling, by distillation,  
 or if fresh rain water be used, the air will always  
 be diminished by the agitation; and this is cer-  
 tainly the fairest method of making the experi-  
 ment. If the water be fresh pump water, there  
 will always be an increase of the air by agitation,  
 the air contained in the water being set loose, and  
 joining

joining that which is in the jar. In this case, also, the air has never failed to be restored; but then it might be suspected that the melioration was produced by the addition of some more wholesome ingredient. As these agitations were made in jars with wide mouths, and in a trough which had a large surface exposed to the common air, I take it for granted that the noxious effluvia, whatever they be, were first imbibed by the water, and thereby transmitted to the common atmosphere. In some cases this was sufficiently indicated by the disagreeable smell which attended the operation.

After I had made these experiments, I was informed that an ingenious physician and philosopher had kept a fowl alive twenty-four hour, in a quantity of air in which another fowl of the same size had not been able to live longer than an hour, by contriving to make the air, which it breathed, pass through no very large quantity of acidulated water, the surface of which was not exposed to the common air; and that even when the water was not acidulated, the fowl lived much longer than it could have done, if the air which it breathed had not been drawn through the water. As I should not have concluded that this experiment would have succeeded so well, from any observations that I had made upon the subject, I took a quantity of air in which mice had died, and agitated it very strongly, first in about five times its own quantity of distilled water, in the manner in which I had impregnated water with fixed air; but though the operation was continued a long time, it made no sensible change in the properties of the air. I also repeated the operation with

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pump water, but with as little effect. In this case, however, though the air was agitated in a phial, which had a narrow neck, the surface of the water in the bafon was considerably large, and exposed to the common atmosphere, which must have tended a little to favour the experiment. In order to judge more precisely of the effect of these different methods of agitating air, I transferred the very noxious air, which I had not been able to amend in the least degree by the former method, into an open jar, standing in a trough of water; and when I had agitated it till it was diminished about one third, I found it to be better than air, in which candles had burned out, as appeared by the test of the nitrous air; and a mouse lived in 2  $\frac{1}{2}$  ounce measures of it a quarter of an hour, and was not sensibly affected the first ten or twelve minutes.

In order to determine whether the addition of any *acid* to the water, would make it more capable of restoring putrid air, I agitated a quantity of it in a phial containing very strong vinegar; and after that in *aqua fortis*, only half diluted with water; but, by neither of these processes was the air at all mended, though the agitation was repeated at intervals during a whole day, and it was moreover allowed to stand in that situation all night.

Since, however, water in these experiments must have imbibed and retained a certain portion of the noxious effluvia, before they could be transmited to the external air, I do not think it improbable but that the agitation of the sea and large lakes may be of some use for the purification of the atmosphere, and the putrid matter contained in water may be imbibed

imbibed by aquatic plants, or be deposited in some other manner.

Having found, by several experiments above-mentioned, that the proper putrid effluvium is something quite distinct from fixed air, and finding, by the experiments of Dr. Macbride, that fixed air corrects putrefaction ; I once concluded that this effect was produced, not by stopping the flight of the fixed air, or restoring to the putrefying substance the very same thing that had escaped from it ; and which was the common vinculum of all its parts (which is that ingenious author's hypothesis) but by an affinity between the fixed air and the putrid effluvium. It therefore occurred to me, that fixed air, and air tainted with putrefaction, though equally noxious when separate, might make a wholesome mixture, the one correcting the other ; and I was confirmed in this opinion by, I believe, not less than fifty or sixty instances, in which air, that had been made in the highest degree noxious, by respiration or putrefaction, was so far sweetened, by a mixture of about four times as much fixed air that afterwards mice lived in it exceedingly well, and in some cases almost as long as in common air. I found it, indeed, to be more difficult to restore old putrid air by this means ; but I hardly ever failed to do it, when the two kinds of air had stood a long time together, by which I mean about a fortnight or three weeks.

The reason why I do not absolutely conclude that the restoration of air in these cases was the effect of fixed air, is that, when I made a trial of the mixture, I sometimes agitated the two kinds

of air pretty strongly together, in a trough of water, or at least passed it several times through the water, from one jar to another, that the superfluous fixed air might be absorbed, not suspecting at that time that the agitation could have any other effect; but having since found that very violent, and especially long continued agitation in water, without any mixture of fixed air, never failed to render any kind of noxious air in some measure fit for respiration (and in one particular instance the mere transferring of the air from one vessel to another through the water, though for a much longer time than I ever used for the mixtures of air, was of considerable use for the same purpose); I began to entertain some doubt of the efficacy of fixed air, for that purpose. In some cases also the mixture of fixed air had by no means so much effect on the putrid air as, from the generality of my observations, I should have expected.

I was always aware, indeed, that it might be said, that, the residuum of fixed air not being very noxious, such an addition must contribute to mend the putrid air; but, in order to obviate this objection, I once mixed the residuum of as much fixed air as I had found, by a variety of trials, to be sufficient to restore a given quantity of putrid air, with an equal quantity of putrid air, without making any sensible melioration of it.

Upon the whole, I am inclined to think that this process could hardly have succeeded so well as it did with me, and in so great a number of trials, unless fixed air have some tendency to correct air tainted with respiration or putrefaction; and it is

perfectly agreeable to the analogy of Dr. Macbride's discoveries, and may naturally be expected from them, that it should have such an effect.

By a mixture of fixed air I have made wholesome the residuum of air generated by putrefaction only, from mice plunged in water. This, one would imagine, *a priori*, to be the most noxious of all kinds of air. For if common air only tainted with putrefaction be so deadly, much more might one expect that air to be so, which was generated from putrefaction only; but it seems to be nothing more than common air tainted with putrefaction, and therefore requires no other process to sweeten it. In this case, however, we seem to have an instance of the generation of genuine common air, though mixed with something that is foreign to it. Perhaps the residuum of fixed air may be another instance of the same nature.

Fixed air is equally diffused through the whole mass of any quantity of putrid air with which it is mixed; for dividing the mixture into two equal parts, they were reduced in the same proportion by passing through water. But this is also the case with some of the kinds of air which will not incorporate, as inflammable air, and air in which brimstone has burned.

If fixed air tend to correct air which has been injured by animal respiration or putrefaction, limekilns, which discharge great quantities of fixed air, may be wholesome in the neighbourhood of populous cities, the atmosphere of which must abound with putrid effluvia. I should think also that physicians might avail themselves of the application

of

of fixed air in many putrid disorders, especially as it may be so easily administered by way of clyster, where it would often find its way to much of the putrid matter. Nothing is to be apprehended from the distention of the bowels by this kind of air, since it is so readily absorbed by any fluid or moist substance. Since fixed air is not noxious *per se*, but, like fire, only in excess, I do not think it at all hazardous to attempt to breathe it. It is however easily conveyed into the stomach, in natural or artificial Pyrmont water, in briskly fermenting liquors, or a vegetable diet. It is possible, however, that a considerable quantity of fixed air might be imbibed by the absorbing vessels of the skin, if the whole body, except the head, should be suspended over a vessel of strongly fermenting liquor; and in some putrid disorders this treatment might be very salutary. If the body was exposed quite naked, there would be very little danger from the cold in this situation, and the air having freer access to the skin might produce a greater effect. Being no physician, I run no risk by throwing out these random, and perhaps whimsical, proposals.

Having communicated my observations on fixed air, and especially my scheme of applying it by way of clyster in putrid disorders, to Mr. Hey, an ingenious surgeon in this town, a case recently occurred, in which he had an opportunity of giving it a trial; and mentioning it to Dr. Hird and Dr. Crowther, two physicians who attended the patient, they approved the scheme, and it was put in execution: both by applying the fixed air by way of clyster, and at the same time making the patient

patient drink plentifully of liquors strongly impregnated with it. The event was such, that I requested Mr. Hey to draw up a particular account of the case, describing the whole of the treatment, that the public might be satisfied that this new application of fixed air is perfectly safe, and also have an opportunity of judging how far it had the effect which I expected from it; and as the application is new, and not unpromising, I shall beg leave to subjoin his letter to me on the subject, by way of *Appendix* to these papers.

## V.

OR AIR IN WHICH A MIXTURE OF BRIMSTONE  
AND FILINGS OF IRON HAS STOOD.

Finding in Dr. Hales's account of his experiments, that there was a great diminution of the quantity of air in which a mixture of powdered brimstone and filings of iron, made into a paste with water, had stood, I repeated the experiment, and found the diminution greater than I had expected. The diminution of air by this process is made as effectually, and as expeditioufly, in quicksilver as in water; and it may be measured with the greatest accuracy, because there is neither any previous expansion nor increase of the quantity of air, and because it is some time before it begins to have any sensible effect. The diminution of air by this process is various; but I have generally

generally found it to be between  $\frac{1}{7}$  and  $\frac{2}{7}$  of the whole.

Air thus diminished is not heavier, but rather lighter than common air; and though lime-water does not become turbid when it is exposed to this air, it is probably owing to the formation of a selenitic salt, as was the case with the simple burning of brimstone above mentioned. That something proceeding from the brimstone strongly affects the water which is confined in the same place with this brimstone, is manifest from the very strong smell that it has of the volatile spirit of vitriol. I conclude the diminution of air by this process is of the same kind with the diminution of it in the other cases, because when this mixture is put into air which has been previously diminished, either by the burning of candles, by respiration, or putrefaction, though it never fails to diminish it something more, it is, however, no farther than this process alone would have done it. If a fresh mixture be introduced into a quantity of air which had been reduced by a former mixture, it has little or no farther effect.

I observed, that when a mixture of this kind was taken out of a quantity of air in which a candle had before burned out, and in which it had stood for several days, it was quite cold and black, as it always becomes in a confined place; but it presently grew very hot, smoked copiously, and smelted very offensively; and when it was cold, it was brown, like the rust of iron.

I once put a mixture of this kind to a quantity of inflammable air, made from iron, by which means it was diminished  $\frac{1}{7}$  or  $\frac{2}{7}$  in its bulk; but, as far as I could

I could judge, it was still as inflammable as ever. Another quantity of inflammable air was also reduced in the same proportion, by a mouse putrefying in it; but its inflammability was not seemingly lessened.

Air diminished by this mixture of iron filings and brimstone, is exceedingly noxious to animals, and I have not perceived that it grows any better by keeping in water. The smell of it is very pungent and offensive.

The quantity of this mixture which I made use of in the preceding experiments, was from two to four ounce measures; but I did not perceive, but that the diminution of the quantity of air (which was generally about twenty ounce measures) was as great with the smallest, as with the largest quantity. How small a quantity is necessary to diminish a given quantity of air to a *maximum*, I have made no experiments to ascertain.

As soon as this mixture of iron filings, with brimstone and water, begins to ferment, it also turns black, and begins to swell, and it continues to do so, till it occupies twice as much space as it did at first; and the force with which it expands is great; but how great it is I have not endeavoured to determine.

When this mixture is immersed in water, it generates no air, though it becomes black, and swells.

VI.  
OF NITROUS AIR.

Ever since I first read Dr. Hales's most excellent *Statistical Essays*, I was particularly struck with that experiment of his, of which an account is given, VOL. I. p. 224, and Vol. II. p. 280; in which common air, and air generated from the Walton pyrites, by spirit of nitre, made a turbid red mixture, and in which part of the common air was absorbed; but I never expected to have the satisfaction of seeing this remarkable appearance, supposing it to be peculiar to that particular mineral. Happening to mention this subject to the Hon. Mr. Cavendish, when I was in London, in the Spring of the year 1772, he said that he did not imagine but that other kinds of pyrites might answer as well as that which Dr. Hales made use of, and that probably the red appearance of the mixture depended upon the spirit of nitre only. This encouraged me to attend to the subject; and having no pyrites, I began with the solution of the different metals in spirit of nitre, and catching the air which was generated in the solution, I presently found what I wanted, and a good deal more.

Beginning with the solution of brass, on the 4th of June 1772, I first found this remarkable species of air; one effect of which, though it was casually observed by Dr. Hales, he gave but little attention to; and which, as far as I know, has passed altogether unnoticed since his time, inasmuch that no name has been given to it. I therefore found myself, contrary

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to my first resolution, under an absolute necessity of giving a name to this kind of air myself. When I first began to speak and write of it to my friends, I happened to distinguish it by the name of nitrous air, because I had procured it by means of spirit of nitre only; and though I cannot say that I altogether like the term, because this air is not got from all the metals by the same spirit, neither myself nor any of my friends, to whom I have applied for the purpose, have been able to hit upon a better; so that I am obliged, after all, to content myself with it.

I have found that this kind of air is readily procured from iron, copper, brass, tin, silver, quicksilver, bismuth, and nickel, by the nitrous acid only, and from gold and the regulus of antimony by aqua regia. The circumstances attending the solution of each of these metals are various, but hardly worth mentioning, in treating of the properties of the air which they yield, which, from what metal soever it is extracted, has, as far as I have been able to observe, the very same properties.

One of the most conspicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a considerable heat. The smell of it, also, is very strong, and remarkable, but very much resembling that of smoking spirit of nitre.

The diminution of a mixture of this and common air is not an equal diminution of both the kinds, which is all that Dr. Hales could observe, but of the common air chiefly, though not wholly. For if one measure of nitrous air be put to two measures of

common air, in a few minutes (by which time the effervescence will be over, and the mixture will have recovered its transparency) there will want about one ninth of the original two measures. I hardly know any experiment that is more adapted to amaze and surprize than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is diminished by it. If, after this full saturation of common air with nitrous air, more nitrous air be put to it, it makes an addition equal to its own bulk, without producing the least redness, or any other visible effect.

That this diminution is chiefly in the quantity of common air, is evident from this observation, that if the smallest quantity of common air be put to any larger quantity of nitrous air, though the two together will not occupy so much space as they did separately, yet the quantity will be still larger than that of the nitrous air only. One ounce measure of common air being put to near twenty ounce measures of nitrous air, made an addition to it of about half an ounce measure. This, however, being a much greater proportion than the diminution of common air, in the former experiment, seems to prove that part of the diminution in the former case is in the nitrous air. Besides, it will presently appear, that nitrous air is subject to a most remarkable diminution; and as common air, in a variety of other cases, suffers a diminution from one fifth to one fourth, I conclude, that in this case also it does not exceed that proportion, and therefore that the remainder of the diminution respects the nitrous air,

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In order to judge whether the water contributed to the diminution of this mixture of nitrous and common air, I made the whole process several times in quicksilver, using one third of nitrous, and two thirds of common air, as before. In this case the redness continued a very long time, and the diminution was not so great as when the mixtures had been made in water, there remaining one seventh more than the original quantity of common air. This mixture stood all night upon the quicksilver; and the next morning I observed that it was no farther diminished upon the admission of water to it, nor by pouring it several times through the water, and letting it stand in water two days. Another mixture, which stood about six hours on the quicksilver, was diminished a little more upon the admission of water, but was never less than the original quantity of common air. In another case, however, in which the mixture stood but a very short time in quicksilver, the farther diminution, which took place upon the admission of water, was much more considerable; so that the diminution, upon the whole, was very nearly as great as if the process had been entirely in water. It is evident from these experiments, that the diminution is in part owing to the absorption by the water; but that when the mixture is kept a long time, in a situation in which there is no water to absorb any part of it, it acquires a constitution, by which it is afterwards incapable of being absorbed by water.

In order to determine whether the fixed part of common air was deposited in the diminution of it by

by nitrous air, I inclosed a vessel full of lime water in the jar in which the process was made, but it occasioned no precipitation of the lime; and when the vessel was taken out, after it had been in that situation a whole day, the lime was easily precipitated by breathing into it as usual.

It is exceedingly remarkable that this effervescence and diminution, occasioned by the mixture of nitrous air, is peculiar to common air, or air fit for respiration; and, as far as I can judge, from a great number of observations, is at least very neatly, if not exactly, in proportion to its fitness for this purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by putting mice, or any other animals, to breathe in it. This was a most agreeable discovery to me, as I hope it may be an useful one to the public; especially as, from this time, I had no occasion for so large a stock of mice as I had been used to keep for the purpose of these experiments, using them only in those which required to be very decisive; and in these cases I have seldom failed to know beforehand in what manner they would be affected.

It is also remarkable that, on whatever account air is unfit for respiration, this same test is equally applicable. Thus there is not the least effervescence between nitrous and fixed air, or inflammable air, or any species of diminished air. Also the degree of diminution being from nothing at all to more than one third of the whole of any quantity of air, we are by this means in possession of a prodigiously large scale, by which we may distinguish very

very small degrees of difference in the goodness of air. I have not attended much to this circumstance, having used this test chiefly for greater differences; but, if I did not deceive myself, I have perceived a real difference in the air of my study, after a few persons have been with me in it, and the air on the outside of the house. Also a phial of air having been sent me, from the neighbourhood of York, it appeared not to be so good as the air near Leeds; that is, it was not diminished so much by an equal mixture of nitrous air, every other circumstance being as nearly the same as I could contrive. It may perhaps be possible, but I have not yet attempted it, to distinguish some of the different winds, or the air of different times of the year, by this test.

By means of this test I was able to determine what I was before in doubt about, *viz.* the kind as well as the degree of injury done to air by candles burning in it. I could not tell with certainty by means of mice, whether it was at all injured with respect to respiration; and yet if nitrous air may be depended upon for furnishing an accurate test, it must be rather more than one third worse than common air, and have been diminished by the same general cause of the other diminutions of air. For when, after many trials, I put one measure of thoroughly putrid and highly noxious air, into the same vessel with two measures of good wholesome air, and into another vessel an equal quantity, *viz.* three measures of air in which a candle had burned out; and then put equal quantities of nitrous air to each of them, the former was diminished rather more than the latter. It agrees with

any kind of air that has been diminished, and rendered noxious by any other process; but when it is put to a quantity of nitrous air, it diminishes it so much, that no more than one fourth of the original quantity will be left. The effect of this process is generally perceived in five or six hours, about which time the visible effervescence of the mixture begins; and in a very short time it advances so rapidly, that in about an hour almost the whole effect will have taken place. If it be suffered to stand a day or two longer, the air will still be diminished farther, but only a very little farther, in proportion to the first diminution. The glass jar, in which the air and this mixture have been confined, has generally been so much heated in this process, that I have not been able to touch it.

Nitrous air thus diminished has not the peculiar smell of nitrous air, but smells just like common air in which the same mixture has stood; and it is not capable of being diminished any farther, by a fresh mixture of iron and brimstone.

Common air saturated with nitrous air is also no farther diminished by this mixture of iron filings and brimstone, though the mixture ferments with great heat, and swells very much in it.

Plants die very soon, both in nitrous air, and also in common air saturated with nitrous air, but especially in the former.

Neither nitrous air, nor common air saturated with nitrous air, differs in specific gravity from common air, or, at least, so little, that I could not

not be sure of it, sometimes about three pints of it seeming to be about half a grain heavier, and at other times as much lighter than common air.

Having, among other kinds of air, exposed a quantity of nitrous air, to water out of which the air had been well boiled, in the experiment to which I have more than once referred, as having been the occasion of several new and important observations, I found that  $\frac{4}{5}$  of the whole was absorbed. Perceiving, to my great surprize, that so very great a proportion of this kind of air was miscible with water, I immediately began to agitate a considerable quantity of it, in a jar standing in a trough of the same kind of water; and with about four times as much agitation as fixed air requires, it was so far absorbed by the water, that only about one fifth remained. This remainder extinguished flame, and was noxious to animals. Afterwards I diminished a pretty large quantity of it to one eighth of its original bulk, and the remainder still retained much of its peculiar smell, and diminished common air a little. A mouse also died in it, but not so suddenly as it would have done in pure nitrous air. In this operation the peculiar smell of nitrous air is very manifest, the water being first impregnated with the air, and then transmitting it to the common atmosphere.

This experiment gave me the hint of impregnating water with nitrous air, in the manner in which I had before done it with fixed air; and I presently found that distilled water would imbibe about one-tenth of its bulk of this kind of air, and that

that it acquired a remarkably acid and astringent taste from it. The smell of water thus impregnated is at first peculiarly pungent. I did not chuse to swallow any of it, though, for any thing that I know, it may be perfectly innocent, and perhaps, in some cases, salutary.

This kind of air is retained very obstinately by water. In an exhausted receiver a quantity of water thus saturated emitted a whitish fume, such as sometimes issues from bubbles of this air when it is first generated, and also some air bubbles; but though it was suffered to stand a long time in this situation, it still retained its peculiar taste; but when it had stood all night pretty near the fire, the water was become quite vapid, and had deposited a filmy kind of matter, of which I had often collected a considerable quantity from the trough in which jars containing this air had stood. This I suppose to be a precipitate of the metal by the solution of which the nitrous air was generated. I have not given so much attention to it as to know, with certainty, in what circumstances this deposit is made, any more than I do the matter deposited from inflammable air abovementioned; for I cannot get it, at least in any considerable quantity, when I please; whereas I have often found abundance of it, when I did not expect it at all.

The nitrous air with which I made the first impregnation of water was extracted from copper; but when I made the impregnation with air from quick-silver, the water had the very same taste, though the matter deposited from it seemed to be of a dif-

ferent kind ; for it was whitish, whereas the other had a yellowish tinge. Except the first quantity of this impregnated water, I could never deprive any more that I made of its peculiar taste. I have even let some of it stand more than a week, in phials with their mouths open, and sometimes very near the fire, without producing any alteration in it.

Whether any of the spirit of nitre be properly contained in the nitrous air, and be mixed with the water in this operation, I have not yet endeavoured to determine. This, however, may probably be the case, as the spirit of nitre is in a considerable degree volatile.

It will perhaps be thought, that the most useful, if not the most remarkable, of all the properties of this extraordinary kind of air, is its power of preserving animal substances from putrefaction, and of restoring those that are already putrid, which it possesses in a far greater degree than fixed air. My first observation of this was altogether casual. Having found nitrous air to suffer so great a diminution as I have already mentioned by a mixture of iron filings and brimstone, I was willing to try whether it would be equally diminished by other causes of the diminution of common air, especially by putrefaction ; and for this purpose I put a dead mouse into a quantity of it, and placed it near the fire, where the tendency to putrefaction was very great. In this case there was a considerable diminution, *viz.* from  $5\frac{1}{4}$  to  $3\frac{1}{2}$  ; but not so great as I had expected, the antiseptic power of the nitrous air having checked the

the tendency to putrefaction; for when, after a week, I took the mouse out, I perceived, to my very great surprize, that it had no offensive smell.

Upon this I took two other mice, one of them just killed, and the other soft and putrid, and put them both into the same jar of nitrous air, standing in the usual temperature of the weather, in the months of July and August of 1772; and after 25 days, having observed that there was little or no change in the quantity of the air, I took the mice out; and, examining them, found them both perfectly sweet, even when cut through in all places. That which had been put into the air when just dead was quite firm; and the flesh of the other, which had been putrid and soft, was still soft, but perfectly sweet.

In order to compare the antiseptic power of this kind of air with that of fixed air, I examined a mouse which I had inclosed in a phial full of fixed air, as pure as I could make it, and which I had corked very close; but upon opening this phial in water, about a month after, I perceived that a large quantity of putrid effluvium had been generated; for it rushed with violence out of the phial; and the smell that came from it, the moment the cork was taken out, was insufferably offensive. Indeed Dr. Macbride says, that he could only restore very thin pieces of putrid flesh by means of fixed air. Perhaps the antiseptic power of these kinds of air may be in proportion to their acidity. If a little pains were taken with this subject, this remarkable antiseptic power of nitrous air, might possibly be applied to various uses, perhaps to the preservation

preservation of the more delicate birds, fishes, fruits, &c. mixing it in different proportions with common or fixed air. Of this property of nitrous air anatomists may perhaps avail themselves, as animal substances may by this means be preserved in their natural soft state; but how long it will answer for this purpose, experience only can shew.

I calcined lead and tin in the manner hereafter described in a quantity of nitrous air, but with very little sensible effect; which rather surprized me; as, from the result of the experiment with the iron filings and brimstone, I had expected a very great diminution of the nitrous air by this process, the mixture of iron filings and brimstone, and the calcination of metals, having the same effect upon common air, both of them diminishing it in nearly the same proportion.

Nitrous air is procured from all the proper metals by spirit of nitre, except lead, and from all the semi-metals that I have tried, except zinc. For this purpose I have used bismuth and nickel, with spirit of nitre only, and regulus of antimony and platina, with aqua regia.

I got little or no air from lead by spirit of nitre, and have not yet made any experiments to ascertain the nature of this solution. With zinc I have taken a little pains.

Four penny weights and seventeen grains of zinc dissolved in spirit of nitre, to which as much water was added, yielded about twelve ounce measures of air, which had, in some degree, the properties of nitrous air, making a slight effervescence with common air, and diminishing it about as much as nitrous

trous air, which had been itself diminished one half by washing in water. The smell of them both was also the same; so that I concluded it to be the same thing, that part of the nitrous air which is imbibed by water being retained in this solution.

In order to discover whether this was the case, I made the solution boil in a sand heat. Some air came from it in this state, which seemed to be the same thing, as nitrous air diminished about one sixth, or one eighth, by washing in water. When the fluid part was evaporated, there remained a brown fixed substance, which was observed by Mr. Hellot, who describes it, Ac. Par. 1735, M. p. 35. A part of this I threw into a small red hot crucible; and covering it immediately with a receiver, standing in water, I observed that very dense red fumes rose from it, and filled the receiver. This redness continued about as long as that which is occasioned by a mixture of nitrous and common air; the air was also considerably diminished within the receiver. This substance, therefore, must certainly have contained within it the very same thing, or principle, on which the peculiar properties of nitrous air depend. It is remarkable, however, that though the air within the receiver was diminished about one fifth by this process, it was itself as much affected with a mixture of nitrous air, as common air is, and a candle burnt in it very well. This may perhaps be attributed to some effect of the spirit of nitre, in the composition of that brown substance.

Nitrous air, I find, will be considerably diminished in its bulk by standing a long time in water,

ter, about as much as inflammable air is diminished in the same circumstances. For this purpose I kept for some months a quart bottle full of each of these kinds of air; but as different quantities of inflammable air vary very much in this respect, it is not improbable but that nitrous air may vary also.

From one trial that I made, I conclude that nitrous air may be kept in a bladder much better than most other kinds of air. The air to which I refer was kept about a fortnight in a bladder, through which the peculiar smell of the nitrous air was very sensible for several days. In a day or two the bladder became red, and was much contracted in its dimensions. The air within it had lost very little of its peculiar property of diminishing common air.

I did not endeavour to ascertain the exact quantity of nitrous air produced from given quantities of all the metals which yield it; but the few observations which I did make for this purpose I shall recite in this place:

dwt. gr.		
6	0	of silver yielded
5	19	of quicksilver
1	2 $\frac{1}{2}$	of copper
2	0	of brass
0	20	of iron
1	5	of bismuth
0	12	of nickel
		17 $\frac{1}{2}$ ounce measures
		4 $\frac{1}{2}$
		14 $\frac{1}{2}$
		21
		16
		6
		4

## VII. OF

## VII.

## OF AIR INFECTED WITH THE FUMES OF BURNING CHARCOAL.

Air infected with the fumes of burning charcoal is well known to be noxious; and the Honourable Mr. Cavendish favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 ounce measures, by passing through a red-hot iron tube filled with the dust of charcoal. This diminution he ascribed to such a destruction of common air as Dr. Hales imagined to be the consequence of burning. Mr. Cavendish also observed, that there had been a generation of fixed air in this process, but that it was absorbed by sope leys. This experiment I also repeated, with a small variation of circumstances, and with nearly the same result.

Afterwards, I endeavoured to ascertain, by what appears to me to be an easier and a more certain method, in what manner air is affected with the fumes of charcoal, viz. by suspending bits of charcoal within glass vessels, filled to a certain height with water, and standing inverted in another vessel of water, while I threw the focus of a burning mirror, or lens, upon them. In this manner I diminished a given quantity of air one fifth, which is nearly in the same proportion with other diminutions of air.

Some fixed air seems to be contained in charcoal, and to be set loose from it by this process; for if I made use of lime-water, it never failed to become

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turbid, presently after the heat was applied. This was the case with whatever degree of heat the charcoal had been made. If, however, the charcoal had not been made with a very considerable degree of heat, there never failed to be a permanent addition of inflammable air produced ; which agrees with what I observed before, that, in converting dry wood into charcoal, the greatest part is changed into inflammable air. I have sometimes found, that charcoal which was made with the most intense heat of a smith's fire, which vitrified part of a common crucible in which the charcoal was confined, and which had been continued above half an hour, did not diminish the air in which the focus of a burning mirror was thrown upon it ; a quantity of inflammable air equal to the diminution of the common air being generated in the process ; whereas, at other times, I have not perceived that there was any generation of inflammable air, but a perfect diminution of common air, when the charcoal had been made with a much less degree of heat. This subject deserves to be farther investigated.

To make the preceding experiment with still more accuracy, I repeated it in quicksilver ; when I perceived that there was a small increase of the quantity of air, from a generation either of fixed or inflammable air, but I suppose of the former. Thus it stood without any alteration a whole night, and part of the following day ; when lime-water, being admitted to it, it presently became turbid, and, after some time, the whole quantity of air, which was about four ounce measures, was diminished one fifth, as before. In this case, I carefully weighed the piece of charcoal, which was exactly two grains, and could not find that

that it was sensibly diminished in weight by the operation.

Air thus diminished by the fumes of burning charcoal not only extinguishes flame, but is in the highest degree noxious to animals; it makes no effervescence with nitrous air, and is incapable of being diminished any farther by the fumes of more charcoal, by a mixture of iron filings and brimstone, or by any other cause of the diminution of air that I am acquainted with.

This observation, which respects all other kinds of diminished air, proves that Dr. Hales was mistaken in his notion of the absorption of air in those circumstances in which he observed it. For he supposed that the remainder was, in all cases, of the same nature with that which had been absorbed, and that the operation of the same cause would not have failed to produce a farther diminution; whereas all my observations not only shew that air, which has once been fully diminished by any cause whatever, is not only incapable of any farther diminution, either from the same or from any other cause, but that it has likewise acquired new properties, most remarkably different from those which it had before, and that they are, in a great measure, the same in all the cases. These circumstances give reason to suspect, that the cause of diminution is, in reality, the same in all the cases. What this cause is, may, perhaps, appear in the next course of observations,

## VIII.

## OF THE EFFECT OF THE CALCINATION OF METALS, AND OF THE EFFLUVIA OF PAINT MADE WITH WHITE-LEAD AND OIL, ON AIR.

Having been led to suspect, from the experiments which I had made with charcoal, that the diminution of air in that case, and perhaps in other cases also, was, in some way or other, the consequence of its having more than its usual quantity of phlogiston, it occurred to me, that the calcination of metals, which are generally supposed to consist of nothing but a metallic earth united to phlogiston, would tend to ascertain the fact, and be a kind of *experimentum crucis* in the case. Accordingly, I suspended pieces of lead and tin in given quantities of air, in the same manner as I had before treated the charcoal ; and throwing the focus of a burning mirror or lens upon them, in such a manner as to make them fume copiously, I presently perceived a diminution of the air. In the first trial that I made, I reduced four ounce measures of air to three, which is the greatest diminution of common air that I had ever observed before, and which I account for, by supposing that, in other cases, there was not only a cause of diminution, but causes of addition also, either of fixed or inflammable air, or some other permanently elastic matter, but that, the effect of the calcination of metals being simply the escape of phlogiston, the cause of diminution was alone and uncontroled.

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The air, which I had thus diminished by calcination of lead, I transferred into another clean phial, but found that the calcination of more lead in it had no farther effect upon it. This air also, like that which had been infected with the fumes of charcoal, was in the highest degree noxious, made no effervescence with nitrous air, was no farther diminished by the mixture of iron filings and brimstone, and was not only rendered innoxious, but also recovered, in a great measure, the other properties of common air, by washing in water.

It might be suspected that the noxious quality of the air in which lead was calcined, might be owing to some fumes peculiar to that metal; but I found no sensible difference between the properties of this air, and that in which tin was calcined.

The water over which metals are calcined acquires a yellowish tinge, and an exceedingly pungent smell and taste, pretty much, as near as I can recollect, for I did not compare them together, like that over which brimstone has been frequently burned. Also a thin and whitish pellicle covered both the surface of the water, and likewise the sides of the phial in which the calcination was made, insomuch that, without frequently agitating the water, it grew so opaque by this constantly accumulating incrustation, that the sun beams could not be transmited through it in a quantity sufficient to produce the calcination.

I imagined, however, that, even when this air was transferred into a clean phial, the metals were not so easily melted or calcined as they were in fresh air; for the air being once fully saturated with phlogiston, may not so readily admit any more, though it be only

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to transmit it to the water. I also suspected that metals were not easily melted or calcined in inflammable, fixed, or nitrous, air, or any kind of diminished air. None of these kinds of air suffered any change by this operation; nor was there any precipitation of lime, when charcoal was heated in any of these kinds of air standing in lime-water.

Query. May not water impregnated with phlogiston from calcined metals, or by any other method, be of some use in medicine? The effect of this impregnation is exceedingly remarkable; but the principle with which it is impregnated is volatile, and entirely escapes in a day or two, if the surface of the water be exposed to the common atmosphere.

It should seem that phlogiston is retained more obstinately by charcoal than it is by lead or tin; for when any given quantity of air is fully saturated with phlogiston from charcoal, no heat that I have yet applied has been able to produce any more effect upon it; whereas, in the same circumstances, lead and tin may still be calcined. The air, indeed, can take no more; but the water receives it, and the sides of the phial also receive an addition of incrustation. This is a white powdery substance, and well deserves to be examined. I shall endeavour to do it at my leisure.

Lime-water never became turbid by the calcination of metals over it; but the colour, smell, and taste of the water was always changed, and the surface of it became covered with a yellow pellicle, as before.

When this process was made in quicksilver, the air was diminished only one fifth; and upon water being

admitted to it, no more was absorbed; which is an effect similar to that of a mixture of nitrous and common air, which was mentioned before.

The preceding experiments on the calcination of metals suggested to me a method of explaining the cause of the mischief which is known to arise from fresh paint, made with white lead (which I suppose is an imperfect calx of lead) and oil. To verify my hypothesis, I first put a small pot full of this kind of paint, and afterwards (which answered much better, by exposing a greater surface of the paint) I daubed several pieces of paper with it, and put them under a receiver, and observed, that in about twenty-four hours, the air was diminished between one fifth and one fourth, for I did not measure it very exactly. This air also was, as I expected to find it, in the highest degree, noxious; it did not effervesce with nitrous air, it was no farther diminished by a mixture of iron filings and brimstone, and was made wholesome by agitation in water deprived of all air.

I think it appears pretty evident, from the preceding experiments on the calcination of metals, that air is some way or other diminished in consequence of being highly charged with phlogiston, and that agitation in water restores it, by imbibing a great part of the phlogistic matter. That water has a considerable affinity with phlogiston, is evident from the strong impregnation which it receives from it. May not plants also restore air diminished by putrefaction, by absorbing part of the phlogiston with which it is loaded? The greater part of a dry plant, as well as of a dry animal substance, consists of inflammable air, or something that is capable of being converted

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into inflammable air ; and it seems to be as probable that this phlogistic matter may have been imbibed by the roots and leaves of plants, and afterwards incorporated into their substance, as that it is altogether produced by the power of vegetation. May not this phlogistic matter be even the most essential part of the food and support of both vegetable and animal bodies ?

In the experiments with metals, the diminution of air seems to be the consequence of nothing but a saturation with phlogiston ; and in all the other cases of the diminution of air, I do not see but that it may be effected by the same means. When a vegetable or animal substance is dissolved by putrefaction, the escape of the phlogistic matter (which, together with all its other constituent parts, is then let loose from it) may be the circumstance that produces the diminution of the air in which it putrefies. It is highly improbable that what remains after an animal body has been thoroughly dissolved by putrefaction, should yield so great a quantity of inflammable air, as the dried animal substance would have done. Of this I have not made an actual trial, though I have often thought of doing it, and still intend to do it ; but I think there can be no doubt of the result. Again, the iron, by its fermentation with brimstone and water, is evidently reduced to a calx, so that phlogiston must have escaped from it. Phlogiston also must evidently be set loose by the ignition of charcoal, and is not improbably the matter which flies off from paint, composed of white lead and oil. Lastly, since spirit of nitre is known to have a very remarkable affinity with phlogiston, it is far from being

being improbable that nitrous air may also produce the same effect by the same means.

To this hypothesis it may be objected, that, if diminished air be air saturated with phlogiston, it ought to be inflammable; but this by no means follows, since its inflammability may depend upon some particular mode of combination, or degree of affinity, with which we are not acquainted. Besides, inflammable air seems to consist of some other principle, or to have some other constituent part, besides phlogiston and common air, as is probable from that remarkable deposit, which, as I have observed, is made by inflammable air, both from iron and zinc.

It is not improbable, however, but that a greater degree of heat may inflame that air which extinguishes a common candle, if it could be conveniently applied. Air that is inflammable, I observe, extinguishes red hot wood; and indeed inflammable substances can only be those which, in a certain degree of heat, have a less affinity with the phlogiston they contain, than the air, or some other contiguous substance, has with it; so that the phlogiston only quits one substance, with which it was before combined, and enters another, with which it may be combined in a very different manner. This substance, however, whether it be air or any thing else, being now fully saturated with phlogiston, and not being able to take any more, in the same circumstances, must necessarily extinguish fire, and put a stop to the ignition of all other bodies, that is, to the farther escape of phlogiston from them.

That plants restore noxious air, by imbibing the phlogiston with which it is loaded, is very agreeable to

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the conjectures of Dr. Franklin, made many years ago, and expressed in the following extract from the last edition of his Letters, p. 346.

“ I have been inclined to think that the fluid *fire*,  
 “ as well as the fluid *air*, is attracted by plants in  
 “ their growth, and becomes consolidated with the  
 “ other materials of which they are formed, and  
 “ makes a great part of their substance; that, when  
 “ they come to be digested, and to suffer in the  
 “ vessels a kind of fermentation, part of the fire, as  
 “ well as part of the air, recovers its fluid active state  
 “ again, and diffuses itself in the body, digesting and  
 “ separating it; that the fire so reproduced, by di-  
 “ gestion and separation, continually leaving the  
 “ body, its place is supplied by fresh quantities,  
 “ arising from the continual separation; that what-  
 “ ever quickens the motion of the fluids in an ani-  
 “ mal quickens the separation, and re-produces  
 “ more of the fire, as *exercise*; that all the fire  
 “ emitted by wood, and other combustibles, when  
 “ burning, existed in them before, in a solid state,  
 “ being only discovered when separating; that some  
 “ fossils, as sulphur, sea-coal, &c. contain a great  
 “ deal of solid fire; and that, in short, what escapes  
 “ and is dissipated in the burning of bodies, besides  
 “ water and earth, is generally the air, and fire,  
 “ that before made parts of the solid.”

## IX.

### OF AIR PROCURED BY MEANS OF SPIRIT OF SALT.

Being very much struck with the result of an ex-  
 periment of the Hon. Mr. Cavendish, related Phil.  
 Trans.

Trans. Vol. LVI. p. 157. by which, though, he says, he was not able to get any inflammable air from copper, by means of spirit of salt, he got a much more remarkable kind of air, *viz.* one that lost its elasticity by coming into contact with water, I was exceedingly desirous of making myself acquainted with it. On this account, I began with making the experiment in quicksilver, which I never failed to do in any case in which I suspected that air might either be absorbed by water, or be in any other manner affected by it; and by this means I presently got a much more distinct idea of the nature and effects of this curious solution.

Having put some copper filings into a small phial, with a quantity of spirit of salt; and making the air, which was generated in great plenty, on the application of heat, ascend into a tall glass vessel full of quicksilver, and standing in quicksilver, the whole produce continued a considerable time without any change of dimensions. I then introduced a small quantity of water to it, when about three fourths of it (the whole being about four ounce measures) presently, but gradually, disappeared, the quicksilver rising in the vessel. I then introduced a considerable quantity of water; but there was no farther diminution of the air; and the remainder I found to be inflammable.

Having frequently continued this process a long time after the admission of the water, I was much amused with observing the large bubbles of the newly generated air, which came through the quicksilver, the sudden diminution of them when they came to the water, and the very small bubbles which went

through the water. They made, however, a continual, though slow, increase of inflammable air.

Fixed air, being admitted to the whole produce of this air from copper, had no sensible effect upon it. Upon the admission of water, a great part of the mixture, which, no doubt, was the most subtle kind of air from the copper, presently disappeared; another part, which I suppose to have been the fixed air, was absorbed slowly; and in this particular case the very small permanent residuum did not take fire; but it is very possible that it might have done so, if the quantity had been greater.

Lime-water being admitted to the whole produce of air from copper became white; but this I suspect to have arisen from some other circumstance than the precipitation of the lime which it contained.

The solution of lead in the marine acid is attended with the very same phenomena as the solution of copper in the same acid; about three fourths of the generated air disappearing on the contact of water, and the remainder being inflammable.

The solutions of iron, tin, and zinc, in the marine acid, were all attended with the same phenomena as the solutions of copper and lead, but in a less degree; for in iron one eighth, in tin one sixth, and in zinc one tenth of the generated air disappeared on the contact with water. The remainder of the air from iron, in this case, burned with a green, or very light blue flame.

I had always thought it something extraordinary that a species of air should lose its elasticity by the mere contact of any thing, and from the first suspected that it must have been imbibed by the water that

that was admitted to it; but so very great a quantity of this air disappeared upon the admission of a very small quantity of water, that I could not help concluding that appearances favoured the former hypothesis. I found, however, that when I admitted a much smaller quantity of water, confined in a narrow glass tube, a part only of the air disappeared, and that very slowly, and that more of it vanished upon the admission of more water. This observation put it beyond a doubt, that this air was properly imbibed by the water, which, being once fully saturated with it, was not capable of receiving any more. The water thus impregnated tasted very acid, even when it was much diluted with other water, through which the tube containing it was drawn. It even dissolved iron very fast, and generated inflammable air. This last observation, together with another which immediately follows, led me to the discovery of the true nature of this remarkable kind of air, as it had hitherto been called.

Happening, at one time, to use a good deal of copper and a small quantity of spirit of salt, in the generation of this kind of air, I was surprised to find that air was produced long after, I could not but think that the acid must have been saturated with the metal; and I also found that the proportion of inflammable air to that which was absorbed by the water continually diminished, till, instead of being one fourth of the whole as I had first observed, it was not so much as one twentieth. Upon this, I concluded that this subtle air did not arise from the copper, but from the spirit of salt; and presently making the experiment with the acid only, without any copper,

per, or metal of any kind, this air was immediately produced in as great plenty as before; so that this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of spirit of salt, which appear to be of such a nature, that they are not liable to be condensed by cold, like the vapour of water, and other fluids. This vapour, however, seems to lose its elasticity, in some measure, gradually, unless it should be thought to be affected by the quicksilver, with which it is in contact; for it was always diminished, more or less, by standing.

This elastic acid vapour extinguishes flame, and is much heavier than common air; but how much heavier, will not be easy to ascertain. A cylindrical glass vessel, about three fourths of an inch in diameter, and four inches deep, being filled with it, and turned upside down, a lighted candle may be let down into it more than twenty times before it will burn at the bottom. It is pleasing to observe the colour of the flame in this experiment; for both before the candle goes out, and also when it is first lighted again, it burns with a beautifully green, or rather light blue flame, such as is seen when common salt is thrown into the fire.

When this elastic vapour is all expelled from any quantity of spirit of salt, which is easily perceived by the vapour being condensed by cold, the remainder is a very weak acid, barely capable of dissolving iron.

Being now in the possession of a new subject of experiments, *viz.* an elastic acid vapour, in the form of a permanent air, easily procured, and effectually confined by glass and quicksilver, with which

which it did not seem to have any affinity; I immediately began to introduce a variety of substances to it, in order to ascertain its peculiar properties and affinities, and also the properties of those other bodies with respect to it.

Beginning with water, which, from preceding observations, I knew would imbibe it, and become impregnated with it; I found that  $2\frac{1}{2}$  grains of rain water absorbed three ounce measures of this vapour, after which it was increased one third in its bulk, and weighed twice as much as before; so that this concentrated vapour seems to be twice as heavy as rain water. Water impregnated with it makes the strongest spirit of salt that I have seen, dissolving iron with the most rapidity. Consequently, two thirds of the best spirit of salt is nothing more than mere phlegm or water.

Iron filings, being admitted to this vapour, were dissolved by it pretty fast, half of the vapour disappearing, and the other half becoming inflammable air, not absorbed by water. Putting chalk to it, fixed air was produced.

I had not introduced many substances to this vapour, before I discovered that it had an affinity with phlogiston, so that it would deprive other substances of it, and form with it such an union as constitutes inflammable air; which seems to shew, that inflammable air universally consists of the union of some acid vapour with phlogiston.

Inflammable air was produced, when to this vapour I put spirit of wine, oil of olives, oil of turpentine, charcoal, phosphorus, bees-wax, and even sulphur. This last observation, I own, surprised

prized me; for, the marine acid being reckoned the weakest of the three mineral acids, I did not think that it had been capable of dislodging the oil of vitriol from this substance; but I found that it had the very same effect both upon alum and nitre; the vitriolic acid in the former case, and the nitrous in the latter, giving place to the stronger vapour of spirit of salt.

The rust of iron, and the precipitate of nitrous air made from copper, also imbibed this vapour very fast, and the little that remained of it was inflammable air; which proves, that these calces contain phlogiston. It seems also to be pretty evident, from this experiment, that the precipitate above-mentioned is a real calx of the metal, by the solution of which the nitrous air is generated.

As some remarkable circumstances attend the absorption of this vapour of spirit of salt, by the substances above-mentioned, I shall briefly mention them.

Spirit of wine absorbs this vapour as readily as water itself, and is increased in bulk by that means. Also, when it is saturated, it dissolves iron with as much rapidity, and still continues inflammable.

Oil of olives absorbs this vapour very slowly, and, at the same time, it turns almost black, and becomes glutinous. It is also less miscible with water, and acquires a very disagreeable smell. By continuing upon the surface of the water, it became white, and its offensive smell went off in a few days.

Oil of turpentine absorbed this vapour very fast, turning brown, and almost black. No inflammable air was formed, till I raised more of the vapour than the

the oil was able to absorb, and let it stand a considerable time; and still the air was but weakly inflammable. The same was the case with the oil of olives, in the last mentioned experiment; and it seems to be probable, that, the longer this acid vapour had continued in contact with the oil, the more phlogiston it would have extracted from it. It is not improbable, but that, in the intermediate state, before it becomes inflammable air, it may be nearly of the nature of common air.

Bees-wax absorbed this vapour very slowly. About the bigness of a hazel-nut of the wax being put to three ounce measures of the vapour, the vapour was diminished onehalf in two days, and, upon the admission of water, half of the remainder also disappeared. This air was strongly inflammable.

Charcoal absorbed this vapour very fast. About one fourth of it was rendered immiscible in water, and was but weakly inflammable.

A small bit of phosphorus, perhaps about half a grain, smoked, and gave light in the vapour of spirit of salt, just as it would have done in common air confined. It was not sensibly wasted after continuing about twelve hours in that state, and the bulk of the vapour was very little diminished. Water being admitted to it absorbed it as before, except about one fifth of the whole, which was but weakly inflammable.

Putting several pieces of sulphur to this vapour, it was absorbed but slowly. In about twenty-four hours about one fifth of the quantity had disappeared; and water being admitted to the remainder, very little

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more was absorbed. The remainder was inflammable, and burned with a blue flame.

Nowithstanding the affinity which this vapour of spirit of salt appears to have with phlogiston; it is not capable of depriving all bodies of it. I found that dry wood, crusts of bread, and raw flesh, very readily imbibed this acid vapour, but did not part with any of their phlogiston to it. All these substances turned very brown, after they had been some time exposed to this vapour, and tasted very strongly of the acid when they were taken out; but the flesh, when washed in water, became very white, and the fibres easily separated from one another, even more than they would have done if it had been boiled or roasted.

When I put a piece of saltpetre to this vapour, it was presently surrounded with a white fume, which soon filled the whole vessel, exactly like the fume which bursts from the bubbles of nitrous air, when it is generated by a vigorous fermentation, and such as is seen when nitrous air is mixed with this vapour of spirit of salt. In about a minute, the whole quantity of vapour was absorbed, except a very small quantity, which might be the common air that had lodged upon the surface of the spirit of salt within the phial.

A piece of alum exposed to this vapour turned yellow, absorbed it as fast as the saltpetre had done, and was reduced by it to the form of a powder. The surface both of the nitre and alum was, I doubt not, changed into common salt, by this process. Common salt, as might be expected, had no effect whatever on this vapour.

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From considering the affinity which this vapour has with phlogiston, I was induced to try the effect of a mixture of it with nitrous air. Accordingly, to two parts of this vapour, I put one part of nitrous air, and, in about twenty-four hours, the whole was diminished to something less than the original quantity of the vapour, and was no farther diminished by the admission of water. Holding the flame of a candle over this air, the lower part of it burned green, but there was no sensible explosion. At different times I collected  $2\frac{1}{2}$  ounce measures of this mixture of air; but, upon agitating it in rain-water, it was presently diminished to  $1\frac{1}{2}$  ounce measures. In this state it effervesced with nitrous air, and was considerably diminished by it, but not so much as common air. Some allowance, no doubt, must be made for the small quantities of common air, which lodged on the top of my phials, when I raised the sume from the spirit of salt; but, from the precautions that I made use of, I think that very little is to be allowed to this circumstance; and, upon the whole, I am of opinion, that this experiment is an approach to the generation of common air, or air fit for respiration.

I had also imagined, that if air diminished by the processes above-mentioned was affected in this manner, in consequence of its being saturated with phlogiston, a mixture of this vapour might imbibe that phlogiston, and render it wholesome again; but I put about one fourth of this vapour to a quantity of air in which metals had been calcined, without making any sensible alteration in it. I do not, however, infer from this, that air is not diminished by means of phlogiston, since the air, like some other substances,

may hold the phlogiston too fast, to be deprived of it by this acid vapour.

I shall conclude my account of these experiments with observing, that the electric spark is visible in the vapour of spirit of salt, exactly as it is in common air; and though I kept making this spark a considerable time in a quantity of it, I did not perceive that any sensible alteration was made in it. A little inflammable air was produced, but not more than might have come from the two iron nails which I made use of in taking the sparks.

## X.

## MISCELLANEOUS OBSERVATIONS.

Many of the preceding observations relating to the vinous and putrefactive fermentations, I had the curiosity to endeavour to ascertain in what manner the air would be affected by the acetous fermentation. For this purpose I inclosed a phial full of small beer in a jar standing in water, and observed that during the first two or three days there was an increase of the air in the jar, but from that time it gradually decreased, till at length there appeared to be a diminution of about  $\frac{1}{4}$  of the whole quantity. During this time the whole surface of it was gradually covered with a scum, beautifully corrugated. After this there was an increase of the air till there was more than the original quantity; but this must have been fixed air, not incorporated with the rest of the mass; for, withdrawing the beer, which I found to be sour, after it had stood 18 or 20 days under the jar, and passing

passing the air several times through cold water, the original quantity was diminished about  $\frac{1}{4}$ . In the remainder a candle would not burn, and a mouse would have died presently. The smell of this air was exceedingly pungent, but different from that of the putrid effluvium. A mouse lived perfectly well in this air, thus affected with the acetous fermentation; after it had stood several days mixed with four times the quantity of fixed air.

All the kinds of factitious air on which I have yet made the experiment are highly noxious to animals, except that which is extracted from salt-petre, or alum; but in this even a candle burned just as in common air. In one quantity which I got from salt-petre a candle not only burned, but the flame was increased, and something was heard like a hissing, similar to the decrepitation of nitre in an open fire. This experiment was made when the air was fresh made, and while it probably contained some particles of nitre, which would have been deposited afterwards. The air was extracted from these substances by putting them into a gun barrel, which was much corroded and soon spoiled by the experiment. What effect this circumstance may have had upon the air I have not considered.

November 6, 1772, I had the curiosity to examine the state of a quantity of this air, which had been extracted from salt-petre above a year, and which at first was perfectly wholesome; when, to my very great surprize, I found that it was become, in the highest degree, noxious. It made no effervescence with nitrous air, and a mouse died the moment it was put into it. I had not, however, washed it in rain water quite ten minutes. (and;

(and perhaps less time would have been sufficient) when I found, upon trial, that it was restored to its former perfectly wholesome state. It effervesced with nitrous air as much as the best common air ever does, and even a candle burned in it very well, which I had never before observed of any kind of noxious air meliorated by agitation in water. This series of facts, relating to air extracted from nitre, appear to me to be very extraordinary and important, and, in able hands, may lead to considerable discoveries.

There are many substances which impregnate the air in a very remarkable manner, but without making it noxious to animals. Among other things I tried volatile alkaline salts, and camphire, the latter of which I melted with a burning glass, in air inclosed in a phial. The mouse which was put into this air sneezed and coughed very much, especially after it was taken out; but it presently recovered, and did not appear to have been sensibly injured.

Having made several experiments with a mixture of iron filings and brimstone, kneaded to a paste with water, I had the curiosity to try what would be the effect of substituting brass dust in the place of the iron filings. The result was, that when this mixture had stood about three weeks, in a given quantity of air, it had turned black, but was not increased in bulk. The air also was neither sensibly increased nor decreased, but the nature of it was changed, for it extinguished flame, it would have killed a mouse presently, and was not restored by fixed air, which had been mixed with it several days.

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I have frequently mentioned my having, at one time, exposed equal quantities of different kinds of air in jars standing in boiled water. The common air in this experiment was diminished four sevenths, and the remainder extinguished flame. This experiment demonstrates that water does not absorb air equally, but that it decomposes it, taking one part, and leaving the rest. To be quite sure of this fact, I agitated a quantity of common air in boiled water, and when I had reduced it from eleven ounce measures to seven, I found that it extinguished a candle, but a mouse lived in it very well. At another time a candle barely went out when the air was diminished one third, and at other times I have found this effect take place at other very different degrees of diminution. This difference I attribute to the differences in the state of the water with respect to the air contained in it; for sometimes it had stood longer than at other times before I made use of it. I also used distilled water, rain water, and water out of which the air had been pumped, promiscuously with rain water. I even doubt not but that, in a certain state of the water, there might be no sensible difference in the bulk of the agitated air, and yet at the end of the process it would extinguish a candle, air being supplied from the water in the place of that part of the common air which had been absorbed.

It is certainly a little extraordinary that the very same process should so far mend putrid air, as to reduce it to the standard of air in which candles have burned out; and yet that it should so far injure common and wholesome air, as to reduce it to about

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the same standard : but so the fact certainly is. If air extinguish flame in consequence of its being previously saturated with phlogiston, it must, in this case, have been transferred from the water to the air.

To a quantity of common air, thus diminished by agitation in water, till it extinguished a candle, I put a plant, but it did not so far restore it as that a candle would burn in it again ; which to me appeared not a little extraordinary, as it did not seem to be in a worse state than air in which candles had burned out, and which had never failed to be restored by the same means. I had no better success with a quantity of permanent air ; which I had collected from my pump water. Indeed these experiments were begun before I was acquainted with that property of nitrous air, which makes it so accurate a measure of the goodness of other kinds of air ; and it might perhaps be rather too late in the year when I made the experiments. Having neglected these two jars of air, the plants died and putrefied in both of them ; and then I found the air in them both to be highly noxious, and to make no effervescence with nitrous air.

I found that a pint of my pump water contains about one fourth of an ounce measure of air, one half of which was afterwards absorbed by standing in fresh pump water. A candle would not burn in the air, but a mouse lived in it very well. Upon the whole, it seemed to be in about the same state as air in which a candle had burned out.

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I once imagined that, by mere stagnation, air might become unfit for respiration, or at least for the burning of candles; but if this be the case, and the change be produced gradually, it must require a long time for the purpose. For on the 22d of September 1772, I examined a quantity of common air, which had been kept in a phial, without agitation, from May 1771, and found it to be in no respect worse than fresh air, even by the test of the nitrous air.

The crystallization of nitre makes no sensible alteration in the air in which the process is made. For this purpose I dissolved as much nitre as a quantity of hot water would contain, and let it cool under a receiver, standing in water.

November 6, 1772, a quantity of inflammable air, which, by long keeping, had come to extinguish flame, I observed to smell very much like common air in which a mixture of iron filings and brimstone had stood. It was not, however, quite so strong, but it was equally noxious.

Bismuth and nickel are dissolved in the marine acid with the application of a considerable degree of heat; but little or no air is got from either of them; but, what I thought a little remarkable, both of them smelled very much like Harrowgate water. This smell I have met with several times in the course of my experiments, and in processes very different from one another.

As I generally made use of mice in the experiments which relate to respiration, and some persons may chuse to repeat them after me, and pursue them farther than I have done; it may be

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of use to them to be informed, that I kept them without any difficulty in glass receivers, open at the top and bottom, and having a quantity of paper, or tow, in the inside, which should be changed every three or four days; when it will be most convenient also to change the vessel, and wash it. But they must be kept in a pretty exact temperature, for either much heat or much cold kills them presently. The place in which I have generally kept them is a shelf over the kitchin fire place, where, as it is usual in Yorkshire, the fire never goes out; so that the heat varies very little; and I find it to be at a medium about 70 degrees of Fahrenheit's thermometer. When they had been made to pass through the water, as they necessarily must be, in order to a change of air, they require, and will bear a very considerable degree of heat, to warm and dry them.

I found, to my great surprize, in the course of these experiments, that mice will live intirely without water; for though I have kept some of them for three or four months, and have offered them water several times, they would never taste it; and yet they continued in perfect health and vigour. Two or three of them will live very peaceably together in the same vessel; though I had one instance of one mouse tearing another almost in pieces, though there was plenty of provisions for both of them.

The apparatus with which the principal of the preceding experiments were made is exceedingly simple, and cheap. The drawing annexed exhibits a view of every thing that is most important in it.

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A is an oblong trough, about eight inches deep, kept nearly full of water, and B, B are jars standing in it, about ten inches long, and two and a half wide; such as I have generally used for electrical batteries.

C, C are flat stones, sunk about an inch, or half an inch, under the water, on which vessels of any kind may be conveniently placed, during a course of experiments.

D, D are pots nearly full of water, in which jars or phials, containing any kind of air, to which plants or any other substances may be exposed, and having their mouths immersed in water; so that the air in the inside can have no communication with the external air.

E is a small glass vessel, of a convenient size for putting a mouse into it, in order to try the wholesomeness of any kind of air that it may contain.

F is a cylindrical glass vessel, five inches in length, and one in diameter, very proper for trying whether any kind of air will admit a candle to burn in it. For this purpose a bit of wax candle, G, may be fastened to the end of a wire, H, and turned up in such a manner as to be let down into the vessel with the flame upwards. The vessel should be kept carefully covered till the moment that the candle is admitted to it. In this manner I have frequently extinguished a candle above twenty times in one of these vessels full of air, though it is impossible to dip the candle into it, without giving the external air an opportunity of mixing with it, more or less.

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I is a funnel of glass or tin, which is necessary for transferring air into vessels which have narrow mouths.

K is a glass syphon, which is very useful for drawing air out of a vessel which has its mouth immersed in water, and thereby raising the water to whatever height may be most convenient. I do not think it by any means safe to depend upon a valve at the top of a vessel, which Dr. Hales very often made use of; for, since my first disappointments, I have never thought the communication between the external and internal air sufficiently cut off, unless glass, or a body of water, or, in some cases, quicksilver, have intervened between them.

L is a piece of a gun barrel, closed at one end, having the stem of a tobacco-pipe luted to the other. To the end of this pipe I sometimes fastened a flaccid bladder, in order to receive the air discharged from the substance contained in the barrel; but, when the air was generated slowly, I commonly contrived to put this end of the pipe under a vessel full of water, and standing with its mouth inverted in another vessel of water, that the new air might have a more perfect separation from the external air than a bladder could make.

M is a small phial containing some mixture that will generate air. This air passes through a bent glass tube inserted into the cork at one end, and going under the edge of the jar N at the other; the jar being placed with part of its mouth projecting beyond the flat stones C C, for that purpose.

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## A N APPENDIX,

Containing an account of some experiments made by Mr. Hey, which prove that there is no oil of vitriol in water impregnated with fixed air extracted from chalk by oil of vitriol; and also a letter from Mr. Hey, to Dr. Priestley, concerning the effects of fixed air applied by way of clyster.

**EXPERIMENTS TO PROVE THAT THERE IS NO OIL OF VITRIOL IN WATER IMPREGNATED WITH FIXED AIR.**

It having been suggested, that air arising from a fermenting mixture of chalk and oil of vitriol might carry up with it a small portion of the vitriolic acid, rendered volatile by the act of fermentation; I made the following experiments, in order to discover whether the acidulous taste, which water impregnated with such air affords, was owing to the presence of any acid, or only to the fixed air it had absorbed.

**EXPERIMENT I.**

I mixed a tea-spoonful of fyrup of violets with an ounce of distilled water, saturated with fixed air procured from chalk by means of the vitriolic acid; but neither upon the first mixture, nor after

standing 24 hours, was the colour of the syrup at all changed, except by its simple dilution.

#### EXPERIMENT II.

A portion of the same distilled water, unimpregnated with fixed air, was mixed with the syrup in the same proportion: not the least difference in colour could be perceived betwixt this and the above mentioned mixture.

#### EXPERIMENT III.

One drop of oil of vitriol being mixed with a pint of the same distilled water, an ounce of this water was mixed with a tea-spoonful of the syrup. This mixture was very distinguishable in colour from the two former, having a purplish cast, which the others wanted.

#### EXPERIMENT IV.

The distilled water impregnated with so small a quantity of vitriolic acid having a more agreeable taste than when alone, and yet manifesting the presence of an acid by means of the syrup of violets; I subjected it to some other tests of acidity. It formed curds when agitated with soap, lathered with difficulty, and very imperfectly; but not the least ebullition could be discovered upon dropping in spirit of sal ammoniac, or solution of salt of tartar, though I had taken care to render the latter free from causticity by impregnating it with fixed air.

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## EXPERIMENT V.

The distilled water saturated with fixed air neither effervesced, nor shewed any clouds, when mixed with the fixed or volatile alkali.

## EXPERIMENT VI.

No curd was formed by pouring this water upon an equal quantity of milk, and boiling them together.

## EXPERIMENT VII.

When agitated with soap, this water produced curds, and lathered with some difficulty; but not so much as the distilled water mixed with vitriolic acid in the very small proportion above-mentioned. The same distilled water without any impregnation of fixed air lathered with soap without the least previous curdling. River water, and a pleasant pump water not remarkably hard, were compared with these. The former produced curds before it lathered, but not quite in so great a quantity as the distilled water impregnated with fixed air: the latter caused a stronger curd than any of the others above-mentioned.

## EXPERIMENT VIII.

Apprehending that the fixed air in the distilled water occasioned the coagulation, or separation of the oily part of the soap, only by destroying the causticity of the *lixivium*, and thereby rendering the

union less perfect betwixt that and the tallow, and not by the presence of any acid; I impregnated a fresh parcel of the same distilled water with fixed air, which had passed through half a yard of a wide barometer tube filled with salt of tartar; but this water caused the same curdling with soap as the former had done, and appeared in every respect to be exactly the same.

#### EXPERIMENT IX.

Distilled water saturated with fixed air formed a white cloud and precipitation, upon being mixed with a solution of *jaccharum saturni*. I found likewise, that fixed air, after passing through the tube filled with alkaline salt, upon being let into a phial containing a solution of the metallic salt in distilled water, caused a perfect separation of the lead, in form of a white powder; for the water, after this precipitation, shewed no cloudiness upon a fresh mixture of the substances which had before rendered it opaque.

A Letter

A Letter from Mr. HEY to Dr. PRIESTLEY, concerning the Effects of fixed Air applied by way of Clyster.

Leeds, Feb. 15th, 1772.

Reverend Sir,

Having lately experienced the good effects of fixed air in a putrid fever, applied in a manner, I believe, not heretofore made use of, I thought it proper to inform you of the agreeable event, as the method of applying this powerful corrector of putrefaction took its rise principally from your observations and experiments on factitious air; and now, at your request, I send the particulars of the case I mentioned to you, as far as concerns the administration of this remedy.

January 8, 1772, Mr. Lightbowne, a young gentleman who lives with me, was seized with a fever, which, after continuing about ten days, began to be attended with those symptoms that indicate a putrescent state of the fluids.

18th, His tongue was black in the morning when I first visited him, but the blackness went off in the day-time upon drinking: He had begun to doze much the preceding day, and now he took little notice of those that were about him: His belly was loose, and had been so for some days: his pulse beat 110 strokes in a minute, and was rather low: he was ordered to take twenty five grains of Peruvian bark with five of tormentill root in powder every four hours, and to use red wine and water cold as his common drink.

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19th, I was called to visit him early in the morning, on account of a bleeding at the nose which had come on: he lost about eight ounces of blood, which was of a loose texture: the haemorrhage was suppressed, though not without some difficulty, by means of tents made of soft lint, dipped in cold water strongly impregnated with tincture of iron, which were introduced within the nostrils quite through to their posterior apertures; a method which has never yet failed me in like cases. His tongue was now covered with a thick black pellicle, which was not diminished by drinking: his teeth were furred with the same kind of sordid matter, and even the roof of his mouth and fauces were not free from it: his looseness and stupor continued, and he was almost incessantly muttering to himself: he took this day a scruple of the Peruvian bark with ten grains of tormentill every two or three hours: a starch clyster containing a drachm of the compound powder of bole, without opium, was given morning and evening: a window was set open in his room, though it was a severe frost, and the floor was frequently sprinkled with vinegar.

20th, He continued nearly in the same state: when roused from his dozing, he generally gave a sensible answer to the questions asked him; but he immediately relapsed, and repeated his muttering. His skin was dry, and harsh, but without petechiae. He sometimes voided his urine and faeces into the bed, but generally had sense enough to ask for the bed-pan: as he now nauseated the bark in substance, it was exchanged for Huxham's tincture,

tinctorie, of which he took a table-spoonful every two hours in a cup full of cold water: he drank sometimes a little of the tinctorie of roses, but his common liquors were red wine and water, or rice water and brandy acidulated with elixir of vitriol: before drinking, he was commonly requested to rinse his mouth with water to which a little honey and vinegar had been added. His loofeness rather increased, and the stools were watery, black, and foetid: It was judged necessary to moderate this discharge, which seemed to sink him, by mixing a drachm of the *theriaca Andromachi* with each clyster.

21st. The same putrid symptoms remained, and a *subfultus tendinum* came on: his stools were more foetid; and so hot, that the nurse assured me she could not apply her hand to the bed-pan, immediately after they were discharged, without feeling pain on this account: The medicine and clysters were repeated.

Reflecting upon the disagreeable necessity we seemed to lie under of confining this putrid matter in the intestines, lest the evacuation should destroy the *vis viva* before there was time to correct its bad quality, and overcome its bad effects, by the means we were using; I considered, that, if this putrid ferment could be more immediately corrected, a stop would probably be put to the flux, which seemed to arise from, or at least to be increased by it; and the *fomes* of the disease would likewise be in a great measure removed. I thought nothing was so likely to effect this, as the introduction of fixed air into the alimentary canal,

which, from the experiments of Dr. Macbride, and those you have made since his publication, appears to be the most powerful corrector of putrefaction hitherto known. I recollect what you had recommended to me as deserving to be tried in putrid diseases, I mean, the injection of this kind of air by way of clyster, and judged that in the present case such a method was clearly indicated.

The next morning I mentioned my reflections to Dr. Hird and Dr. Crowther, who kindly attended this young gentleman at my request, and proposed the following method of treatment, which, with their approbation, was immediately entered upon. We first gave him five grains of ipecacoanha, to evacuate in the most easy manner part of the putrid *colluvies*: he was then allowed to drink freely of brisk orange-wine, which contained a good deal of fixed air, yet had not lost its sweetnes: the tincture of bark was continued as before; and the water, which he drank along with it, was impregnated with fixed air from the atmosphere of a large vat of fermenting wort, in the manner I had learned from you: instead of the astringent, air alone was injected, collected from a fermenting mixture of chalk and oil of vitriol: he drank a bottle of orange-wine in the course of this day, but refused any other liquor except water and his medicine: two bladders full of air were thrown up in the afternoon.

23d. His stools were less frequent; their heat likewise and peculiar *faeces* were considerably diminished: his muttering was much abated, and the *subfultus tendimum* had left him. Finding that part of the air was rejected when given with a bladder in

the

the usual way, I contrived a method of injecting it which was not so liable to this inconvenience. I took the flexible tube of that instrument which is used for throwing up the fume of tobacco, and tied a small bladder to the end of it that is connected with the box made for receiving the tobacco, which I had previously taken off from the tube: I then put some bits of chalk into a six ounce phial until it was half filled; upon these I poured such a quantity of oil of vitriol as I thought capable of saturating the chalk, and immediately tied the bladder, which I had fixed to the tube, round the neck of the phial: the cylster pipe, which was fastened to the other end of the tube, was introduced into the *anus* before the oil of vitriol was poured upon the chalk. By this method the air passed gradually into the intestines as it was generated; the rejection of it was in a great measure prevented; and the inconvenience of keeping the patient uncovered during the operation was avoided.

24th, He was so much better, that there seemed to be no necessity for repeating the clysters: the other means were continued. The window of his room was now kept shut.

25th, All the symptoms of putrefcency had left him; his tongue and teeth were clean; there remained no unnatural blackness or *fator* in his stools, which had now regained their proper consistence; his dozing and muttering were gone off; and the disagreeable odour of his breath and perspiration was no longer perceived. He took nourishment to-day, with pleasure; and, in the afternoon, sat up an hour in his chair.

His

His fever, however, did not immediately leave him; but this we attributed to his having caught cold from being incautiously uncovered, when the window was open, and the weather extremely severe; for a cough, which had troubled him in some degree from the beginning, increased, and he became likewise very hoarse for several days, his pulse, at the same time, growing quicker: but these complaints also went off, and he recovered, without any return of the bad symptoms above-mentioned.

I am, Reverend Sir,

Your obliged humble servant,

W<sup>m</sup> Hey.

P. S.

October 29, 1772.

Fevers of the putrid kind have been so rare in this town, and in its neighbourhood, since the commencement of the present year, that I have not had an opportunity of trying again the effects of fixed air, given by way of clyster, in any case exactly similar to Mr. Lightbowne's. I have twice given water saturated with fixed air in a fever of the putrescent kind, and it agreed very well with the patients. To one of them the aerial clysters were administered, on account of a looseness, which attended the fever, though the stools were not black, nor remarkably hot or fetid.

These

These clysters did not remove the looseness, though there was often a greater interval than usual betwixt the evacuations, after the injection of them. The patient never complained of any uneasy distention of the belly from the air thrown up, which, indeed, is not to be wondered at, considering how readily this kind of air is absorbed by aqueous and other fluids, for which sufficient time was given, by the gradual manner of injecting it. Both those patients recovered, though the use of fixed air did not produce a crisis before the period on which such fevers usually terminate. They had neither of them the opportunity of drinking such wine as Mr. Lightbowne took after the use of fixed air was entered upon; and this, probably, was some disadvantage to them.

I find the methods of procuring fixed air, and impregnating water with it, which you have published, are preferable to those I made use of in Mr. Lightbowne's case.

The flexible tube used for conveying the fume of tobacco into the intestines, I find to be a very convenient instrument in this case, by the method before-mentioned (only adding water to the chalk, before the oil of vitriol is instilled, as you direct): the injection of air may be continued at pleasure, without any other inconvenience to the patient, than what may arise from his continuing in one position during the operation, which scarcely deserves to be mentioned, or from the continuance of the clyster-pipe within the anus, which is but trifling, if it be not shaken much, or pushed against the rectum.

When I said in my letter, that fixed air appeared to be the greatest corrector of putrefaction hitherto

known, your philosophical researches had not then made you acquainted with that most remarkably antiseptic property of nitrous air. Since you favoured me with a view of some astonishing proofs of this, I have conceived hopes, that this kind of air may likewise be applied medicinally to great advantage.

W. H.

A CORRECTION.

Upon re-examining Dr. Hales's account of his experiments to measure the diminution of air by respiration (Statical Essays, Vol. I. p. 238, 4th edition), I find an error of the press, of  $\frac{2}{7}$  for  $\frac{1}{7}$ ; so that the diminution of air by respiration, though very various, is, I believe, always considerably less than by putrefaction, or several other causes of diminution. But though I have mentioned this diminution as equal to several others, nothing material depends upon it; the quality of the air thus diminished being, in all respects, the same, notwithstanding the cause of increase (which, as I have observed, in this and other cases, co-operates with the cause of diminution) be greater than I had supposed.

I did not endeavour to measure the quantity of the diminution of air by respiration, as I did that by other causes; because I imagined that it had been done sufficiently by others, and especially by Dr. Hales.

T H E E N D.

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